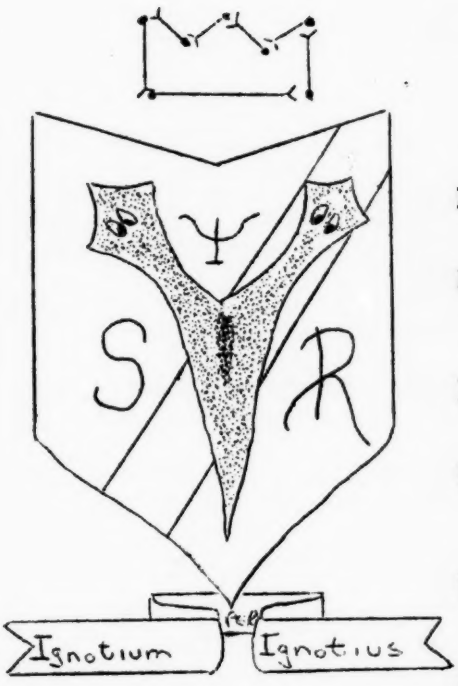

THE WORM RUNNER'S DIGEST

THE RESEARCH MAN'S PRAYER

Help me be MANIC so I may be joyous though the results are equivocal.

Help me be DEPRESSIVE for when a prediction is verified, I must know that it will not later be confirmed.

Help me be SADISTIC so I suffer not though the subjects be sorely anguished.

Help me be MASOCHISTIC for even the most obstinate experimental animal should be a pleasure to me.

Help me be PSYCHOPATHIC to quiet the guilt when I tell loved ones that the experiment is going well.

Help me be SCHIZOPHRENIC to sustain myself by finding hopeful trends in random data.

Help me be PARANOID so I can see in the hostile attitudes of others the proof of the supremacy of my own work.

Help me to have ANXIETY ATTACKS so that even on holidays I find myself toiling in the laboratory.

And finally,

Help my wife get a job! For when I cross over the shadowy border of normalcy, someone will have to support the kids. Amen.

— R. A. McCleary

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THE WORM RUNNER'S DIGEST

An Informal Journal of Comparative Psychology

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THE WORM RUNNER'S DIGEST Vol. II
An Informal Journal of Comparative Psychology

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WORMS AND THINGS

*WHAT is a DIGEST? Should we take our title seriously, and attempt to prune down all manuscripts submitted to us? Or should we, like the Reader's Digest is reputed to have done occasionally, publish articles in "digested" form which actually are longer than in their original, pristine, "undigested" form? Come to think of it, should we concentrate on original articles, or should we from time to time bring our readers papers from other periodicals not likely to be read by those who "subscribe" to this informal journal of comparative psychology?

And what about research reports published for the first time in these pages? Does the scientific ethic proscribe republication elsewhere at a later date? Most scientific journals, as you may know, frown upon duplicate publication, and will not accept an article if a substantial portion if it has appeared elsewhere. Where does this leave the DIGEST? Can we justifiably pressure our fellow worm runners to let us bring our readers "the news" in a hurry, when our publishing such news may preclude their presenting their results formally in another publication? After all, fame and promotions depend upon citable references in one's bibliography. Does one dare cite an article from the WORM RUNNER'S DIGEST?

Upon due and careful consideration of these problems, over a small glass of Cherry Heering, we have decided to continue to be what we always have been--an informal journal. With the continuing financial support of the Atomic Energy Commission and the National Institute of Mental Health (and let us pray such support continues) and the indulgence of the University of Michigan, we shall print what we think will interest our readers, and let it go at that. In this issue, for instance, we bring you a serious article by Professor MICHAEL POLANYI, of Oxford, which will appear at a later date in the world's first psychological journal, MIND. We wish to thank Dr. RYLE, MIND's editor, for his indulgence in this matter. We have also cannibalized another informal journal, THE MALPIGHII, of some light verse and animal nonsense. Like the editor of THE MALPIGHII, N. S. DENMAN, we believe that science is too often a sacred cow and that scientists, on occasion, take themselves and their work over seriously. Our motto is, though, that it is worthwhile poking fun only at worthwhile things. In future issues we will endeavor to keep a proper balance between the proper and the frivolous.

As for the question of "duplicate publication", we'll solve it this way. Instead of bringing you formal articles on research results at other labs, we'll print "abstracts," "preliminary reports," and "informal reports" on what our colleagues are up to. Hopefully, such understanding editors as HARRY HARLOW, of the Journal of Comparative and Physiological Psychology, and the many fine editors of Behavior, will be inclined to wink at these small sins of commission when our authors send along formal reports on their work later on.

And if anyone out there has the gumption to cite the DIGEST in some formal scientific publication, and the skill to sneak such citation past the editor, he has our blessing and our admiration!

*Speaking of research support, and the National Institute of Mental Health, more than a dozen of our readers have written in to tell us that Dr. EDWARD HALAS, of the Department of Psychology at the University of North Dakota, has

received a grant of $18,389.00 from NIMH to study learning in the flatworm. We knew Ed back in the old days, when we were both graduate students at the University of Texas and didn't have a worm to our names, much less $18,389.00. We congratulate Dr. HALAS heartily.

In like fashion, we are informed by Drs. ALLEN CALVIN and PAUL WOODS, of Hollins College in Roanoke, that they are establishing a "planarium" there with support from the National Science Foundation. While far to the north, in Montreal, in D. O. HEBB's laboratory at McGill, strange events are also in progress. One of Professor HEBB's graduate students, FRED HACKER, wrote us asking for a shipment of flatworms of assorted sizes and species so that McGill could have a "planarium" too. We shipped the worms, but discovered later they were halted at the border for lack of passport. Hopefully they reached McGill eventually in live state. We are most pleased at this sending of "coals to Newcastle," since we began our worm work with the notion of finding Hebbian cell assemblies in the beasts, and the "coronet" of neurons atop our shield (see cover) is in honor of Hebb.

*How does one abbreviate ribonuclease? Frankly, we aren't quite sure, but we've made a stab at it in this issue. Consider the inconsistencies of science:

Acetylcholine, thought to be a "transmitter" chemical at the synapse between neurons, is abbreviated ACh. Cholinesterase, which apparently inhibits or breaks up ACh, is abbreviated ChE. Ribonucleic acid, a complex chemical connected with cellular protein synthesis and with heredity, is abbreviated RNA. Ribonuclease breaks up RNA much as ChE breaks up ACh. Should one then abbreviate it RNAE? Or, as is perhaps more common, RNAse? Or, since the "acid" is dropped from the name "ribonuclease," perhaps RNE or RNase? We argued with many local biochemists and pharmacologists, but could establish no consensus. Now, in the November, 1960, issue of our sister publication, The Scientific American, comes an excellent article by SARAH E. STEWART on "The Polyoma Virus." We recommend it to our readers wholeheartedly, for it tells of the difficulties a scientist has when he's foolish enough to adopt an unpopular scientific idea simply because his research indicates it's so. The article also summarizes research on tumors and viruses which utilizes ribonuclease, and glory of glories, the abbreviation found therein is RNA-ASE. We bow our heads in deference to such authority and give thanks the article appeared before this issue of the DIGEST, which contains "a preliminary report" by BILL CORNING at Rochester on the startling effects RNA-ASE apparently has upon retention of a conditioned response in flatworms.

*Visitors to our laboratory, hidden away in the bowels of Mason Hall, are always welcome. In recent months we have put our worms through their paces for such distinguished (and occasionally skeptical) personages as the following: the aforementioned BILL CORNING, Dean EDWARD ERNHART, of Washington University in St. Louis, JAY BOYD BEST, now an Associate Professor at the University of Illinois College of Medicine in Chicago, THOMAS MORRILL of Tallahassee, Florida, LEROY AUGENSTINE, of the Atomic Energy Commission and of Brookhaven National Lab, BERNARD AGRANOFF, late of the National Institutes of Health but now on the staff of the University of Michigan's Mental Health Research Institute, and DANIEL LEHRMAN, of Rutgers University. We were also visited by more than 200 introductory psychology students, singly and in groups, who apparently believed that viewing their senior lecturer's home lab might add a few points to their final grades (sadly enough, it won't!) John Caylor, of the University of California's Riverside Campus, flew in not only to visit for two days, but also to run a few worms with us.

*For those readers who really care, our sister periodical, Science Digest, published a story about our work in its September issue. Net result: So far more than 30 high school students have written in for "worms and things," and we have done our best to supply what we could. Although we have not seen "proof positive," we believe we also appeared in the French publication Science et Vie late this fall. Our DIGEST readers in France are duly alerted.

*IN THIS ISSUE---ROBERT McCLEARY, who provided the "pome" on the cover, offices next to us and researches just down the hall from the worms. For a time, when he was working with fish, the worms were billeted just next to him. We moved when his research threatened to swallow up ours----JOHN H. HOLLIS, of Yerkes Labs at Orange Park, writes that yet another study along the lines of Project Burrow is underway and will be reported in a future issue of the DIGEST----ED ERNHART, who did one of the pioneer studies with flatworms and mazes, is to be congratulated on the arrival of a son. We are informed that the child does not have two heads----MICHAEL POLANYI, one of our favorite people, acts as unofficial publicity agent (self-appointed, bless him) for the worms in Great Britain. He expects to be visiting at the University of Virginia next fall----CARL C. LINDEGREN is Director of the Biological Research Laboratories at Southern Illinois University in Carbondale, and we are delighted that he saw fit to send us the letter, etc., printed in this issue----TOM MORRILL, still our favorite evolutionist, promises to send us some land planaria he has captured on his establishment in Tallahassee---- BEA GELBER, late of the University of Chicago but now President of the Basic Health Research Institute at Tucson, Arizona, has been "conditioning" paramecia even longer than we have been "conditioning" flatworms----BOB SOMMER is currently on a one-man campaign to make scientific articles more readable and more interesting. We wish him luck in what appears to be a hopeless and thankless task. Would that all scientists wrote as wittily and as tellingly as he! ----JOHN GOLWIN writes that he was the high school student "selling worms at 10¢ each" at the Illinois Science Fair whom MAX BRAVERMAN wrote of in the last issue----PETER M. DRIVER and F. E. WARBURTON are both Malpighian members in good standing----BILL CORNING continues to work his usual 14 hour day in the labs at the University of Rochester, where he studies chiefly with Professor ROY JOHN----PAUL and GRETCHEN CORNWELL are charter members of the Planaria Research Group here----This issue was typed, as usual, by our favorite amanuensis, ANGELA THOMPSON----Cover design, as always, by REEVA JACOBSON.

*Good Reading!

James V. McConnell
Editor

TITLE: Knowing and Being

AUTHOR: Michael Polanyi
Merton College
Oxford, England

A few years ago a distinguished psychiatrist demonstrated to his students a patient who was having a mild fit of some kind. Later the class discussed the question whether this had been an epileptic or a hystero-epileptic seizure. The matter was finally decided by the psychiatrist: "Gentlemen", he said, "you have seen a true epileptic seizure. I cannot tell you how to recognize it; you will learn this by more extensive experience."

Clinical practitioners call the peculiar indescribable appearance of a pathological condition its facies; I shall call it a physiognomy, so as to relate it to the delicately varied expressions of the human face which we can likewise identify without being able to tell quite how we recognize them. We may describe as a physiognomy also the peculiar appearance of a species which can be recognised only "esthetically",[1] and further include among physiognomies the characteristics of wines and blends of tea which only experts can recognize.

Any effort to define a physiognomy more explicitly will aim: 1) at identifying its particulars and 2) at describing the relation between these particulars. I shall exemplify these two complementary efforts by reference to some comprehensive entities where the two can be observed separately.

1.) At the time when flying by aeroplane was first developed, around 1914-18, traces of prehistoric settlements were discovered from the air in fields over which many generations had walked without noticing them. Though the aerial photographs clearly revealed the outlines of the sites, the markings on the ground which constituted these outlines frequently remained unrecognisable. Such sites are comprehensive entities that are precisely traceable without mental effort from a distance, while the identification of their particulars at close quarters presents great difficulties. The unspecifiability of particulars is here as complete as it was for a hystero-epileptic fit, but with the difference that the relation of the unspecifiable particulars is clearly definable which it is not in case of a physiognomy.

Usually it is not impossible to identify some particulars of a comprehensive entity, for example some symptoms of a clinically diagnosed disease. But in such cases another limitation of specifiability becomes apparent, as Gestalt-psychology has amply taught us. Specifiability remains incomplete in two ways. First, there is always a residue of particulars left unspecified; and second, even when particulars can be identified, isolation changes their appearance to some extent. Seen in isolation a jumping cat may appear to change its colour from white to dark gray, but when a window is included in the field of vision the cat is seen unchangingly the same white or gray. It has happened in Lancashire that a customer who ordered a shirting of green stripes on a white

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[1] "Esthetic recognition" of C.F.A. Pantin (Science Progress, 42, (1954), p. 412).

ground refused delivery, for he found the ground was purple. Since the colour of any patch of a surface varies with the context in which it is placed, coloured patterns are not specifiable in terms of their isolated particulars.[2]

2.) Now to the reverse effort which aims at specifying the relations of particulars within a comprehensive entity. It is conspicuous in the case of topographic anatomy. We can easily identify the several organs, including blood vessels and nerve tracts in the human body (which is the task of systematic anatomy), but their mutual relation inside the body can be grasped only by a sustained effort of the imagination, based on the partial aspects revealed by successive stages of dissection. Similar difficulties have to be overcome in achieving an understanding of a geological topography and of the spatial relation of the component parts of a complex machine. Even the regular structure of crystals may present such problems. In 1923 H. Mark and I established the atomic structure of white tin. Shortly after that we had a visit by Professor van Arkel from Holland who claimed to have established an entirely different structure. Eventually, it transpired that his structure had the same arrangement of atoms as ours, but that he had described it along lines forming an angle of 45° to those along which we had seen it. This trivial difference in viewing the atomic arrangement had rendered it mutually unrecognisable to both parties, simply because we lacked a sufficient understanding of the relationships involved in the atomic arrangement. To this extent any complex spatial arrangement of opaque objects is unspecifiable.

We can see then two complementary efforts aiming at the elucidation of a comprehensive entity. One proceeds from a recognition of a whole towards an identification of its particulars, the other from the recognition of a group of presumed particulars towards the grasping of their relation in the whole.

I have called these two efforts complementary since they contribute jointly to the same final achievement, yet it is also true that each counteracts the other to some extent at every consecutive step. Every time we concentrate our attention on the particulars of a comprehensive entity, our sense of its coherent existence is temporarily weakened; and every time we move in the opposite direction towards a fuller awareness of the whole, the particulars tend to become submerged in the whole. The concerted advantage of the two processes arises from the fact that normally every dismemberment of a whole adds more to its understanding than is lost through the concurrent weakening of its comprehensive features, and again each new integration of the particulars adds more to our understanding of them than it damages our understanding by somewhat effacing their identity. Thus an alternation of analysis and integration leads progressively to an ever deeper understanding of a comprehensive entity.

Medicine offers readily an illustration of what I have in mind here. A medical student deepens his knowledge of a disease by learning a list of its symptoms with all their variations, but only clinical practice can teach him to integrate the clues observed on an individual patient to form a correct diagnosis of his illness, rather than an erroneous diagnosis which is often more plausible.

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[2]The "red patch" generally used as a paradigm of a primary sense datum can never be unambiguously identified as such.

There is a close analogy between the elucidation of a comprehensive object and the mastering of a skill. A skill too is improved by alternate dismemberment and integration. Sportsmen like skaters, skiers, runners, swimmers; artists like dancers, pianists, or painters; skilled craftsmen and practitioners, all profit from motion studies, followed by a skilful incorporation of the isolated motions into a complete performance.

Moreover, there are limits to the specifiability of skills and these are analogous to those which apply to the specification of physiognomies and other comprehensive entities. The analysis of a skilful feat in terms of its constituent motions remains always incomplete. There are notorious cases, like the distinctive "touch" of a pianist, in which the analysis of a skill has long been debated inconclusively; and common experience shows that no skill can be acquired by learning its constituent motions separately. Moreover, here too isolation modified the particulars: their dynamic quality is lost. Indeed, the identification of the constituent motions of a skill tends to paralyse its performance. Only by turning our attention away from the particulars and towards their joint purpose, can we restore in the isolated motions the qualities required for achieving their purpose. And again, as in the case of knowing a physiognomy, this act of integration is itself unspecifiable. Imitation offers guidance to it, but in the last resort we must rely on discovering for ourselves the right feel of a skilful feat. We alone can catch the knack of it; no teacher can do this for us.

The structural kinship of the arts of knowing and doing is indeed such that they are rarely exercised in isolation; we usually meet a blend of the two. Medical diagnostics combines them about equally. To percuss a lung is as much a muscular feat as a delicate discrimination of the sounds thus elicited. The palpation of a spleen or a kidney combines a skilful kneading of the region with a trained sense for the peculiar feeling of the organ's resistance. It is apposite therefore to include skilful feats among comprehensive entities. Though we may prefer to speak of understanding a comprehensive object or situation and of mastering a skill, we do use the two words nearly as synonyms. Actually, we speak equally of grasping a subject or an art.

A peculiar combination of skilful doing and knowing is present in the working of our sense organs. I have mentioned before the interaction of all parts of the visual field in determining what we see. But it is our own active adjustment of our pupils and lenses and of the convergence of our eyes, that fashion the two retinal images on which this picture is based; and the perceived picture depends on these actions once more as the messages received from the muscles adjusting the eyes are incorporated by us in the qualities of the perceived object. Nor is this all; our perception is effectively co-determined also by messages from the internal ear, from the muscles which keep our body and head in its position, as well as by an ample range of memories. These internal data both guide the reflexes of our eye muscles in shaping our retinal images, and control our evaluation of the sum total of relevant stimuli in terms of perceptions. There is also clear evidence to show that--as might be expected in the case of a skill--the capacity to see objects is acquited by training.

Visual perception does present, therefore, the main characteristics of a combined skilful doing and knowing. But there is also something new here, namely the fact that a major part of the particulars shaping the sight of an

external object are internal actions and stimuli. Though our eye-muscles are subject to intentional control and we might even be dimly aware of a strain in them, such internal particulars are never clearly observable in themselves. And this unspecifiability is not of the same kind as found in the traces of a site which can only be seen jointly from the air; for the particulars of perception have the distinctive peculiarity of being projected from the interior of the body into the space outside it.

A similar projection takes place in the use of tools and probes, and the process can be studied here more easily, since the stimuli that are projected here can be fairly well observed in themselves. The relevant facts are well known. The rower pulling an oar feels the resistance of the water; when using a paper-knife we feel the blade cutting the pages. The actual impact of the tool on our palm and fingers is unspecifiable in the same sense in which the muscular acts composing a skilful performance are unspecifiable; we are aware of them in terms of the tool's action on its object, that is in the comprehensive entity into which we integrate them. But the impacts of a tool on our hands are integrated in a way similar to that by which internal stimuli are integrated to form our perceptions: the integrated stimuli are noticed at a distance removed outward from the point where they impinge on us. In this sense impacts of a tool on our hands function as internal stimuli, and a tool functions accordingly as an extension of our hands. The same is true of a probe used for exploring a cavity, or for a stick by which a blind man feels his way. The impact made by a probe or stick on our fingers is felt at the tip of the probe or stick, where it hits on objects outside, and in this sense the probe or stick is an extension of our fingers that grasp it.

The assimilation of a tool, a stick or a probe to our body is achieved gradually, as its proper use is being learned and perfected. The more fully we master the use of an instrument, the more precisely and discriminatingly will we localise at the further end of it the stimuli impinging on our body while grasping and handling the instrument. This corresponds to the way we learn skilfully to use our eyes to see external objects.

We have now established analogous structures in several processes of knowing: namely, 1) the understanding of physiognomies, 2) the performance of skills, 3) the proper use of sensory organs and 4) the mastery of tools and probes. In order to deal jointly with this whole family we need a general terminology for indicating the relation of a set of particulars to a comprehensive entity. The essential feature is throughout the fact that particulars can be noticed in two different ways. We can be aware of them uncomprehendingly, i.e. in themselves; or understandingly, in their participation in a comprehensive entity. In the first case we focus our attention on the isolated particulars, in the second our attention is directed beyond them to the entity to which they contribute. In the first case therefore we may say that we are aware of the particulars focally, in the second, that we notice them subsidiarily in terms of their participation in a whole.

These alternatives do not necessarily involve any change in the degree of attention given to the particulars. When, after having first looked uncomprehendingly at the symptoms of a patient, we hit on the diagnosis of his illness, his symptoms become meaningful without becoming less noticeable. Focal and subsidiary awareness are definitely not two degrees of attention but two kinds of attention given to the same particulars. In the case of

visual attention we may speak of looking at the particulars in themselves, as distinct from seeing them while looking at the context of which they form part.[3]

We can formulate this difference in terms of meaning. When we focus on a set of particulars uncomprehendingly, they are relatively meaningless, compared with their significance when noticed subsidiarily within the comprehensive entity to which they contribute. As a result we have two kinds of meaning: one exemplified by the particulars of a physiognomy, where the two things, namely a) the isolated particulars and b) what they jointly mean, are not clearly separated in space; and the other, exemplified by visual perception and the use of tools and probes, where the uncomprehended particulars are inside our body or at its surface, and what they mean extends into space outside. It may sometimes prove convenient to call the first kind of meaning physiognostic and the second telegnostic.

Our examples of unspecifiability can now be recapitulated in these terms. When a prehistoric site is discovered from the air we see the meaning of particulars which we cannot see in themselves. On the other hand, the study of topographic anatomy starts by seeing particulars in themselves uncomprehendingly, and then proceeds to realise their spatial relation, which represents their meaning in the present sense of the term, e.g., to a surgeon. In the first case unspecifiability impedes the analysis of a given meaning, in the second case it restricts the discovery of an unknown meaning. In either case the alternation of analysis and integration progressively deepens both our insight into the meaning of a comprehensive entity in terms of its particulars and the meaning of these particulars in terms of their joint significance. When applied to the performance of a skill, this alternation renders our muscular actions ever better adjusted to their joint purpose.

We possess a supremely important system of telegnostic meaning in the denotative meaning of words. A man's name, by itself a meaningless sound, acquires a meaning by being consistently used as a pointer to the person whom it designates; exactly as the knocking of objects by a stick acquires a meaning as we learn to use the stick skilfully for feeling our way in the dark. The use of general denotative terms involves the establishment of a physiognostic meaning, namely the joint meaning of all the instances to which the general term is intended to refer. We meet meanings of analogous structure in the mastery of a skill, in the resourcefulness with which a master handles every new and unprecedented situation.

General conceptions claim a kind of foreknowledge which, though never quite absent from the act of knowing, is more marked in this case than in any other surveyed so far. This offers a convenient transition to the process of discovery in which foreknowledge will be seen manifested even more effectively. I shall lead on to this fact by showing first that all manner of discovery proceeds by a see-saw of analysis and integration similar to that by which our understanding of a comprehensive entity is progressively deepened. The two complementary movements are here a search for the joint meaning of a set of

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[3]But "seeing" and "looking at" cannot be generally used instead of subsidiary and focal noticing.

particulars, alternating with a search for the specification of their hitherto comprehended meaning in terms of yet unknown particulars.

The process of empirical induction may appear to present only the first of these two movements, since it proceeds from the observation of particulars to the discovery of their relations fixed by universal laws. But actually both movements are involved, for advances towards generalisation do alternate with verification. The process of inductive discovery is in fact an oscillation between movements of analysis and integration in which, on balance, integration predominates.

Physics is largely based on such integrative discoveries, while in the progress of biology it is mostly analysis that leads the way. Discoveries aiming at a better understanding of living beings accept for their starting point the existence of plants and animals as complex entities, and usually proceed to analyse these in terms of their organs and the functions of their organs, which are analysed in their turn in terms of physics and chemistry. In biology these particulars are specified as means to an end and this applies also to the physical and chemical study of living beings. Physico-chemical processes may enable plants and animals to function well or else may disturb their functions, and physico-chemical processes form the subject matter of biology only insofar as they have a bearing on life in one of these two ways.

But biological analysis alternates of course with integration and this may even predominate. New comprehensive entities may be established in the manner in which Harvey discovered the circulation of the blood, partly by the observation of valves in veins, or when the process of evolution was established partly from paleozoic data.

We must remember also that great advances have been achieved in physics by pure deduction, when surprising implications of accepted theories were discovered. Indeed, to an important degree all discovery is deductive. For no enquiry can succeed unless it starts from a true, or at least partly true, conception of thennature of things. Such foreknowledge is indispensable and all discovery is but a step towards the verification of such foreknowledge.

The structural analogy between knowledge and skill allows us to expand our perspective from discovery to invention. Foreknowledge which guides discovery has its counterpart in the purpose which guides the invention of a skill, or of a new practical device. The most precious gift of the inventor is his capacity to conceive of a practical purpose which might prove feasible. This is mostly the sole inventive function of a research director, who entrusts to his assistants the solution of the general problems set, or approved, by him. Agog with his problem, the inventor speculates on the possibilities offered by the field of experience, and by his sustained efforts to solve his problem brings about the emergence of its solution. Such is the heuristic power of a practicable problem. It has been beautifully described on an elementary level by Koehler for chimpanzees. He shows how a desire pondered upon by the animal evokes a re-organisation of its field of vision, revealing to it both the instrument and the procedure for satisfying its desire. This is also how skills are acquired and perfected; this how we learn to use tools or probes; how we improve our powers of sensory perception. And we may add, that this is likewise the basic mechanism for solving a mathematical problem. The solution of a problem in mathematics is discovered by casting around with a view to

filling the gaps left open in a situation which constitutes a solvable problem--exactly as a practical problem is solved by an invention.

We can lend greater precision now to what has been said earlier about the way induction is guided by our general conception of the nature of things. Successful induction can be conducted only in the light of a genuine problem. An inductive problem is an intimation of coherence among hitherto uncomprehended particulars and the problem is genuine to the extent to which this intimation is true. Such a surmise vaguely anticipates the evidence which will support it and guides the mind engrossed by it to the discovery of this evidence. This usually proceeds stepwise, the original problem and surmise being modified and corrected by each new piece of evidence; a process which is repeated until eventually some generalisation is accepted as final.

To hit upon a problem is indeed the first step to any discovery and to any creative act. To see a problem is to see something hidden that may yet be accessible. The knowledge of a problem is, therefore, like the knowing of unspecifiables, a knowing of more than you can tell. But our awareness of unspecifiable things, whether of particulars or of the coherence of particulars, is intensified here to an exciting intimation of their hidden presence. It is an engrossing possession of incipient knowledge which passionately strives to validate itself. Such is the heuristic power of a problem.

Yet what is usually called knowledge is structurally similar to the knowledge of a problem. Knowledge is an activity which would be better described as a process of knowing. Indeed, as the scientist goes on enquiring into yet uncomprehended experiences, so do those who accept his discoveries as established knowledge keep applying this to ever changing situations, and to develop it each time a step further. Research is but intensely dynamic knowing, and knowledge but quiet research. Both are ever on the move according to similar principles towards a better understanding of what is already known.

A theory of knowledge must be applicable to both kinds of knowing. Its function must be to justify our reliance on our knowing (whether dynamic or quiet) in spite of its unspecifiable contents. I shall outline this matter by using as my example the recognition of a disease by its physiognomy, which will have also to stand for all the other cases of knowing already surveyed.

The knowledge of a disease is doubly unspecifiable. 1.) We cannot identify a great number of the particulars which we are in fact noticing when we diagnose a case of the disease. 2.) Though we can identify a case of the disease by its typical appearance, we cannot describe it adequately, and there are four closely related reasons for this. a) We are ignorant (according to 1.) of the unspecifiable particulars which would enter into the description. b) The relation between the particulars--even if they could all be identified--could be described only in vague terms which the expert alone would understand. c) Our identification of a disease in any one instance comprehends unspecifiably as its particulars the whole range of cases which, in spite of their individual differences, we have identified in the past, and d) it relies on this comprehension for the future identification of an unlimited number of further cases which might differ from those known before in an infinite variety of unexpected ways. This is its heuristic function.

How can we justify such knowing? Clearly not in terms of its unspecifiable contents ranging over 1) and 2), a), b), c), d). Any justification of it must credit ourselves with unformalisable powers of the kind that Kant acknowledged with reference to subsumptions of the type 2c) or 2d), by calling these powers "an art hidden in the depth of the human soul". We must accredit in particular our competence for comprehending unspecifiable entities, which will yet reveal themselves in the future in an unlimited number of unexpected ways.

This may seem absurd. To claim that we can know the unexpected may appear self-contradictory. It would indeed be self-contradictory if knowing included a capacity to specify what we know. But if all knowledge is fundamentally tacit, as it is if it rests on our subsidiary awareness of particulars in terms of a comprehensive entity, then our knowledge may mean far more than we can tell. The true meaning of the heliocentric system was discovered only by Newton, but it was anticipated 140 years earlier by Copernicus.

We can account for this capacity of ours to know more than we can tell if we believe in the presence of an external reality with which we can establish contact. This I do. I declare myself committed to the belief in an external reality gradually accessible to knowing, and I regard all true understanding as an intimation of such a reality which, being real, may yet reveal itself to our deepened understanding in an indefinite range of unexpected manifestations. I accept the obligation to search for the truth through my own intimations of reality, knowing that there is, and can be, no strict rule by which my conclusions can be justified. My reference to reality legitimates my acts of unspecifiable knowing, even while it duly keeps the exercise of such acts within the bounds of a rational objectivity. For a claim to have made contact with reality necessarily legislates both for myself and others with universal intent.

I must admit that I can fulfil my obligation to serve the truth only to the extent of my natural abilities as developed by my education. No one can transcend his formative milieu very far and beyond this area he must rely on it uncritically. I consider that this matrix of my thought determines my personal calling. It both offers me my opportunity for seeking the truth, and limits my responsibility for arriving at my own conclusions.[4]

This conception of knowledge as personal knowing departs in two closely related respects from the ideal of a strictly justifiable knowledge. It accredits man's capacity to acquire knowledge even though he cannot specify the grounds of his knowing, and it accepts the fact that his knowing is exercised within an accidentally given framework that is largely unspecifiable. These two acceptances are correlated within the effort of integration which achieves knowing. For this effort subsidiarily relies on the one hand on stimuli coming from outside, from all parts of our body and from tools or instruments assimilated to our body, and on the other hand on a wide range of linguistic pointers which bring to bear our pre-conceptions--based on past experiences--on the interpretation of our subject matter. The structure of knowing, revealed by the

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[4]The balance of these two factors is the principal subject matter of my Personal Knowledge, London (Routledge), 1958.

limits of specifiability, thus fuses our subsidiary awareness of the particulars belonging to our subject matter, with the cultural background of our knowing.

To this extent knowing is an indwelling: that is, a utilisation of a framework for unfolding our understanding in accordance with the indications and standards imposed by the framework. But any particular indwelling is a particular form of mental existence. If an act of knowing affects our choice between alternative frameworks, or modifies the framework in which we dwell, it involves a change in our way of being. Since such existential choices would be included in an act of knowing, they could be exercised competently, with universal intent. Nor do similar existential changes, undergone passively, impair the rationality of our personal judgment. They merely affect our calling. For while they modify our opportunities for seeking the truth, they still leave us free to reach our own conclusions within the limits granted by these opportunities. All thought is incarnate; it lives by the body and by the favour of society. But it is not thought unless it strives for truth, which leaves it free to act on its own responsibility, with universal intent.

We have found that our subsidiary awareness of the particulars of a comprehensive entity is fused, in our knowing of the entity, with our subsidiary awareness of our own bodily and cultural being. Let me now attend to the effect exercised by the subsidiary character of these particulars on the kind of being that we ascribe to the entity known through them.

When we watch a man's face and try to fathom his thoughts we do not examine his several features in isolation, but view them jointly as parts of his physiognomy. Thus we are aware of far more particulars, and relations between particulars, than we could specify. Moreover, even the particulars which we could identify would lose their meaning when seen in isolation and would get lost among the irrelevant details of their surroundings. It is generally impossible, therefore, to keep track of a man's mental manifestations, except by watching them as pointers to the mind from which they originate. This is analogously true of any aggregate of unspecifiable particulars forming a comprehensive entity. But it is more pronounced in the traces of a human mind than it is, say, in the traces of a prehistoric site, because a human mind is actively at work in its own traces.

To show this I must go back to glance at the lower levels of life. The moment we identify a plant or animal we attribute an achievement to it. For we recognise it by its typical shape, which it has achieved by growing up healthily. At the same time we will notice any imperfections of its shape. Thus even when considering merely their shapes, living beings can be identified only in terms that attribute success or failure to them as individuals. On this morphological level the centre of individuality is comparatively weak. But the manifestations of this centre become steadily more accentuated as we successively ascend first to the vegetative level of physiological functions, then to active, sentient and appetitive behaviour, and thence again to the level of intelligence and inventiveness, finally reaching the level of the responsible human person. Each time we identify the existence of an individual at one of these levels we thereby attribute to it a measure of success or failure. The moment we recognise a human being we attribute to him a measure of active, responsible intelligence. We know a normal human being as a person, and the particulars of his physiognomy gain a vivid significance by being known in terms of this person.

At whatever level we consider a living being, the centre of its individuality is real. For it is always something we ascertain by comprehending the coherence of largely unspecifiable particulars, and which we yet expect to reveal itself further by an indeterminate range of future manifestations. Thus the criteria of reality are fulfilled. The human mind in particular is real in this sense, and indeed more real than the lower centres of individuality. For an intelligent person's mind is expected to reveal itself over a far wider range of indeterminate manifestations, than his lower centres controlling growth or appetitive action.

The progression from lower to higher levels of individuality involves a fundamental change in the relation between the observed individual and ourselves as observers. When comprehending the lower levels of life in an individual, we ascribe success or failure to it by standards which we deem appropriate to its kind. Throughout the usual compass of biology, we find this relation between the observer and his subject: he is always critical of it. But as our study, ascending to higher levels, reaches that of man, criticism becomes mutual; the subject of our study now criticises us, just as we criticise it. Nor is this the end of the progression: our subject may ascend still further and become our master. We then become apprenticed to our subject and learn to accept its criticism of ourselves.

We arrive here at a process of education that, as such, is akin to the acceptance of our calling and which indeed, forms part of it. For our cultural background is determined to a considerable extent by the influence of a limited number of men. Primitive cultures are transmitted orally by a few authoritative persons in each succeeding generation. Our modern, highly articulate culture flows largely from a small set of men whose works and deeds are revered and consulted for guidance. The knowing of these great men is an <u>indwelling</u> in the sense already defined. Our awareness of their works and deeds serves us (to repeat my definition) as a framework for unfolding our understanding in accordance with the indications and standards imposed by the framework.

This progressive transition is no mere sleight of hand; it is firmly grounded in the co-extension of knowing and being. We can see this by descending gradually from our educational dwelling in the works of great men to the biological study of plants and animals at lower levels. At no stage do we cease to participate in the life of the individual under observation. For we comprehend a living being at all levels by our subsidiary awareness of its particulars. These particulars are never observed in themselves; we <u>read</u> them as manifestations of an individual. We rely on them as pointers, as we rely on a probe or a written text, by making them parts of ourselves for reaching beyond them. Thus our understanding of living beings involves at all levels a measure of indwelling; our interest in passing from biology to the acceptance of our cultural calling, in which we share the life of a human society, including the life of its ancestors, the authors of cultural heritage.

(This article will also appear in <u>Mind</u>. We wish to thank Dr. Ryle, <u>Mind</u>'s editor, for allowing us to publish this article here.--Ed.)

TITLE: The Truth About Mendel, or,
Hollyhocks and the Forebears*

AUTHOR: Peter M. Driver
Community House
College Green
Glouster, England

There happened once a Monk whose name was Father Gregor Mendel, he
was known to be a seeker after deep botanic truth,
but 'tis seldom realised that he was rather temperamentally
inclined towards much lighter things, like pretty girls, forsooth.

He worked with hops and hollyhocks and legumes and Rosaceae
but laboured in his monastery garden not alone,
and if you think my accusations are unfair or crazy, he
produced an offspring; have you never heard of Mendel's-son?

He had a lab' assistant by the name of Sister Mabel-o,
to all the other Monks around his work was just a bore,
she used to help him in the field and at the greenhouse table, oh
this worries me - have I not heard the rhyme somewhere before?

Her golden locks and hollyhocks so prettily did mingle
that he quite forgot Theology and celibacy vows,
but very soon he realized 'twere better to stay single
for they had so many husband-wifely horiticultural rows.

So as his Mabel's abdomen was waxing rather spherical
he packed his extra sandals and departed at a run,
in order to escape the many criticisms clerical
and loud monastic scandals that he knew were bound to come.

He fled through field and forest 'til he reached the Bernese Oberland,
the terror of the local girls, so gossip would insist,
then set up as a florist, having had his name made over and
became plain "Mr. Mendel, A.F., Horticulturist."

Then thinking of the lesson he had learned from Sister Mabel
and young lovers lack of confidence in spells and witches charms,
he advertised <u>The Mendel A.F. Periodic Table</u>,
and proceeded to collect a fortune, plus a coat of arms.

Now every upper schoolboy knows of Mendel's work genetical
and how he wrestled mightily with round and wrinkled peas,
but seldom have they heard of this accomplishment heretical -
the founding of <u>Eugenics</u>, in a life of sloth and ease.

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*Stolen, more or less by permission, from THE MALPIGHII, the bi-weekly newsletter of The Malpighian Society of Montreal, Canada.

TITLE: The Cold War in Biology

AUTHOR: Professor Carl C. Lindegren
Director, Biological Research Laboratory
Southern Illinois University
Carbondale, Illinois

(The following communication from Professor Lindegren is presented in its entirety, including enclosures. We believe that the forthcoming publication of Professor Lindegren's book will stand as a landmark in evolutionary theory. We will pass along further information--i.e., the date the book will be published--as we obtain it. We will also endeavor to bring our readers one or more reviews of this obviously controversial but equally obviously important magnum opus as soon as possible.--Ed.)

* * * * *

Dear Dr. McConnell:

I have been interested in your WORM RUNNER'S DIGEST and thought you might be interested in the preface for a new book which I have just completed entitled THE COLD WAR IN BIOLOGY. I am sending you a copy of the preface and the table of contents which I hope you will find interesting and which, if you wish, you may copy in the DIGEST.

Sincerely yours,

/s/ Carl C. Lindegren

Carl C. Lindegren, Director
Biological Research Laboratory

THE COLD WAR IN BIOLOGY

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Preface

We are all engrossed in the political and economic Cold War between the U.S.A. and the U.S.S.R. There is also a biological-scientific Cold War in which these two countries are the principal protagonists. I shall attempt to describe the causes and effects of this struggle. I cannot do this from the viewpoint of a completely detached observer, but I can do it from the point of view of one who has not been indoctrinated (brainwashed) by either side. I am acquainted with many of the personalities by study of their writings and by personal contact. My attitudes toward most of them is clearly defined and should be known to the reader to guide him in evaluating the nature of my prejudices: I loved Bridges; I honored Morgan; I revere Sturtevant and Oparin; I respect Muller; I admire Beadle, and I believe that Burbank was not and that Lysenko is not a charlatan, although neither of the latter two were trained or familiar with formal biology.

I believe the biological struggle is being fought over differences that are pointless and meaningless,--that the entire affair is a colossal blunder based on the choice of antithetical slogans both of which are ridiculous. The difference of opinion concerns the question of whether or not "acquired" characters are inherited. It is, of course, obvious that the acquired ability to

speak French is not inherited and equally obvious that unless the first living organism had the complete potential for all the seemingly limitless variability that exists among living things, some "acquired" characters must have become "inherited". (It is symbolic of the confusion that neither protagonist has defined "acquired".) We are faced not with the question of whether "acquired" characters <u>are</u> inherited but of <u>how</u> acquired characters <u>become</u> inherited, since acquired characters <u>must</u> have become incorporated into the hereditary apparatus. If a review of this struggle can bring about a reconciliation or a diminution in the hostilities, which have been aggravated by the intemperate arguments (if they can be honored with this reputable term) of indoctrinated protagonists of each side, some progress will have been made. The struggle has had truly tragic consequences in Russia because one proponent of a scientific disagreement managed to obtain the unqualified approval of a ruthless dictator. But there are many indications that in the U.S.A. there is a sufficiency of petty dictators who would have been willing to impose their wills upon those who differed from them and that only the peculiar passion of the American biologists for conformity (a desire to be identified with the majority and a willingness to accept the majority view in spite of inner uncertainties), the open structure of our society, with its diverse opportunities for employment (as well as the easy availability of the comparatively greater freedom of Canada), that prevented the consequences in the U.S.A. from becoming equally tragic.

TITLE: An Informal Report on Two Heads Being Better Than One*

AUTHOR: Dean Edward Ernhart
Department of Psychology
Washington University
St. Louis, Missouri

A Dorothy Parker poem with which to preface this, entitled

Thought for a Sunshiny Morning:

It costs me never a stab nor squirm
To tread by chance upon a worm.
"Aha, my little dear," I say,
"Your clan will pay me back one day."

I seem to recall having promised you, albeit somewhat rashly, to provide you with a brief report of the study on two-headed worms. Being in the process of assembling the material for presentation at our general colloquium here at Washington University, I feel compelled to act on an obviously hasty convention pledge.

The study consists of two experiments, each having the same experimental design. In each, three groups of 5 planaria were used. One group consisted of Normal Control animals (NC), distinguished only by the fact that they were roughly matched in length to the other subjects. The second group, the Surgical Controls (SC), consisted of animals sectioned according to the procedure described in Professor Hamburger's Manual of Experimental Embryology for the production of two-headed planaria, but in which the sections were allowed to grow back together, regenerating a single head. The third group, the Experimental group, consisted of animals formed by the above technique, whereby the head is sectioned at the auricles, the head is discarded, and the animal is then split down the body trunk to approximately the pharynx. If these portions of the animal are kept separated so as not to grow back together, each forms a new head on the end. I kept these parts separated just enough to form two heads on the end of a single "neck", so to speak.

I followed your conditioning procedure with the following exceptions: 40 trials per day instead of 50, this being necessitated by my teaching, etc. schedule at the time; criterion of 17 conditioned responses out of 20 consecutive trials instead of 23 out of 25; AC line voltage controlled through a Variac and thence through a stimulus controller (I threw the output of an inductorium on the scope and considered the resulting waveform to represent a challenge to Fourier himself); stainless steel electrodes extending over the top and into the bottom at the ends of a 3/8 in. deep, 1/4 in. wide and 11 in. long trough cut out of a block of clear plastic.

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*This report arrived at the Editor's desk in the form of a letter from Dean Ernhart. It is printed more or less as it arrived, including asides to the audience and moot questions. Ed.

The results for the first experiment, in which the animals were given three weeks regeneration, follow, giving trials to criterion for each animal, mean and sigma for each group, summary of a hastily done analysis of variance, and the results of the appropriate "t" tests.

	NC	SC	E
	98	157	139
	109	188	165
	130	201	173
	127	166	169
	111	172	180
Mean	115.00	176.80	165.20
Sigma	12.85	15.77	14.01

Source	SS	df	MS	F	
Between	10789.73	2	5394.87		
Within	2933.60	12	244.47	22.07	P < .001
Total	13723.33	14			

Comparison	"t"
SC vs E	1.10*
SC vs NC	6.08**
E vs NC	5.28**

* not significant
** significant at the .01 level of confidence

While hardly startling, these results were in accord with my expectations. However, since earlier work from both the Mich. and W.U. labs had indicated that four and three weeks, respectively, might be insufficient to allow for complete "behavioral" regeneration, I decided to repeat the experiment, but with six weeks regeneration time instead of three. The results follow:

	NC	SC	E
	115	153	97
	147	126	117
	128	122	87
	149	131	108
	117	145	113
Mean	131.20	135.40	104.40
Sigma	14.43	11.74	10.98

Source	SS	df	MS	F	
Between	2828.13	2	1414.07		
Within	2333.20	12	194.43	7.27	P < .001
Total	5161.33	14			

Comparison	"t"
SC vs NC	less than 1
SC vs E	3.86*
NC vs E	2.95*

* significant at the .05 level of confidence

Part of the six-week results I had expected, part surpised me. For example, on the basis of previous difficulties encountered using "old" animals, i.e., planaria which had been around the lab for a considerable period of time, I was not suprised by the poorer performance of the NC Ss at six weeks as contrasted with those of three weeks, although this difference was not significant using a two-tailed test. The extent to which the SC animals closed the gap with the NC animals by six weeks was gratifying, reinforcing my determination to determine the function relating conditionability to length of regeneration period. However, I will confess to some surprise and consternation concerning the behavior of the E animals. I guess I had convinced myself that two heads could not be better than one. At any rate, replications are in order, hopefully soon.

With this I hope you will be contented, at least for the moment. Which reminds me, I think I promised to write up the replications of the maze work for you also. I'll try to get to it after the mid-semester grade reports, warning letters, interviews, etc., are over.

In signing off, I must gleefully point out that two-headed planaria have two sets of eye spots in addition to two sets of cephalic ganglia. While I haven't yet worked carefully through the conditioning literature, I suspect that the additional set of photo-receptors may very well play a role in the apparent superior conditionability of the two-headed creatures. This means running the experiment with a different CS. Any suggestions?

TITLE: A Preliminary Report on the Effects of RNA-ASE on Retention in the Flatworm*

AUTHOR: William Corning**
Department of Psychology
The University of Rochester
Rochester, New York

Earlier studies on the retention of a conditioned response by the tail sections of regenerated flatworms have suggested that the mechanism of retention in the tail may not be primarily neural. Since the "memory" often carries over into several successive asexual generations, there is reason to believe that some form of "cytoplasmic inheritance" of the conditioned response may take place in flatworms. It seemed possible, for example, that the establishment of a CR in the flatworm might in some way effect a change in the RNA in some cells in the animal's posterior regions. At Professor Roy John's suggestion, we undertook to test the effects of RNA-ASE upon the retention of a CR in flatworms.

First, it was necessary to establish what effects RNA-ASE might have on untrained, regenerating flatworms. Stated over-simply, RNA-ASE produces a hydrolysis of RNA (that is, breaks up RNA by adding water to it) and thereby destroys an essential participant in protein synthesis in the cell. Sections of flatworms allowed to regenerate in strong solutions of RNA-ASE often do not regenerate properly--i.e., regenerate into headless, eyeless or misshapen organisms. After several attempts, we discovered that solutions of less than .1 milligrams of RNA=ASE per cc. of pond water yielded apparently normal regenerates. We therefore weakened the solution even more, and our experimental animals were allowed to regenerate in a solution strength of .07 milligrams of RNA-ASE per cc. of pond water. While fewer of the experimental animals survived regeneration than did the controls, the only noticeable effect this solution strength had was to retard complete morphological regeneration by some two or three days.

One experimental and several control groups were then run. The experimental animals were first trained to criterion in a standard light-shock conditioning apparatus. They were then cut in half and allowed to regenerate in the weak solution of RNA-ASE described above. After regeneration was complete (about 16 days), both head and tail regenerates were first given a series of extinction trials (i.e., trials of light alone with no shock) until they were responding to the light at a chance level. Then all animals were retrained to the original criterion to test for any "savings" of the conditioned response.

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*This study, suggested and directed by Professor Roy John at the University of Rochester, will be presented in greater detail in Professor John's section in the 1961 Annual Review of Physiology due out the first part of 1961.

**This report was phoned in long distance to the DIGEST's editor by Mr. Corning. Any mistakes made in transcribing the message and presenting it here are the editor's, and not Mr. Corning's.

The first control group we shall call the pond-water control group. Animals in this group were treated just as were the experimental animals, except that they were allowed to regenerate in pure pond water rather than in the weak solution of RNA-ASE. Following regeneration, both heads and tails were first extinguished, then retrained to criterion.

The regeneration-control animals were treated as follows: Untrained animals were cut in half, allowed to regenerate in pond water, then given "extinction" trials to light alone, then given their first light-shock conditioning trials and trained to criterion.

The RNA-ASE regeneration-control animals were untrained organisms treated in identical fashion to the ordinary regeneration control animals, except that the RNA-ASE regeneration-control animals were allowed to regenerate in the weak solution of RNA-ASE before being given the "extinction" trials to light alone and then being given their first conditioning trials.

The RNA-ASE uncut control animals were first trained to criterion, then put in the weak solution of RNA-ASE for 16 days without being cut, then given extinction and retraining trials.

RESULTS

The experimental animals, the pond-water control animals and the RNA-ASE uncut control animals all reached criterion on their original training in some 500 trials. After cutting and regeneration, the pond-water heads, the pond-water tails, and the experimental heads showed considerable retention of the conditioned response. All of these animals showed an initial high level of responding during the extinction trials, and all retrained to criterion the second time with a savings of about 50%. The experimental tails were another matter, however. These tails showed little retention of the CR no matter how tested. They showed a low, chance-level responding rate during the extinction trials, and it took the experimental tails almost the identical number of trials to "retrain" to criterion as it had taken the original experimental animals during the original training.

Both heads and tails from the ordinary and from the RNA-ASE regeneration-control animals showed much the same sort of behavior as did the experimental tails. That is, all these control animals showed chance-level responding during the "extinction" trials (their first exposure to light) and took some 500 light-shock conditioning trials to reach criterion following "extinction."

DISCUSSION

The results of this experiment indicate tentatively that RNA-ASE has a dramatic effect on the retention of a CR in the tail sections of regenerating animals but little or no effect upon regenerating heads. This may well mean that the physiological mechanism of retention is different in different parts of the flatworm's body. Professor Ralph Gerard has suggested that "memory" in the flatworm's head may be due to a "wiring diagram change," while the tail "retains" the conditioned response because the conditioning experience somehow effects a change in the biochemistry of cells in the animal's tail. These biochemical changes may or may not be closely connected with some part of the

hereditary mechanism (RNA), for there are alternative explanations of our data. Our study does give us better reason to hope we are closer than ever before to being able to specify the mechanism by which an acquired response is passed on from one asexual generation of flatworm to another. Further experiments, now in progress, may help to clarify the issue.

TITLE: Einstein's Girl Friday

AUTHOR: Robert Sommer
The Saskatchewan Hospital
Weyburn, Canada

On whose shoulders does the productivity of a research laboratory rest--the scientist? the administrator? the foundation sponsor? All of these undoubtedly carry some weight but by far the most important member of a scientific team is the secretary. Without her the scientist's dreams remain illegible scrawls on envelope covers and the backs of advertisements. She compels the researcher to translate his ideas into words and paragraphs, makes him conscious of the deficiencies of his jargon and she personifies the outside world in his laboratory. If he cannot convince her of the worth of his ideas, then he might as well return to his instruments and try again.

But the secretary is far more than a critical audience. She is the final and necessary stage in the production of the scientist's bread-and-butter, a published paper. It is axiomatic that a lab can produce only as fast as its secretary can type. The lab with one scientist and four secretaries is in a more favorable position than the lab with four scientists and one secretary. In the latter case, the scientist will spend most of his day peering over the secretary's shoulder seeing whether she is showing favoritism to his three colleagues. There will be intrigues and quarrels to get letters typed, income tax forms filled out, and lead pencils requisitioned. Realistically there is no motivation for the scientist to write if there is no one to type for him. In the lab with one scientist and four secretaries our man is in constant turmoil to keep his assistants busy. He feels he must produce so that his harem looks busy. If they start bringing in knitting and learning bridge, his superiors will become suspicious.

When four or five people share the same secretary, everyone hovers over the secretary's desk to see if she is typing his work rather than someone else's. When one scientist observes a colleague approaching the secretary's desk, he lays down his pencil and becomes all ears to see if the other person isn't trying to slip in a piece of work ahead of his own material. The scientist wants to see that the priority list for typing is maintained. This has a very destructive effect upon morale and performance. It is also responsible for nervous and unhappy secretaries who don't particularly relish the prospect of people peering over their shoulders.

The law of scientific output for any laboratory will be as follows:

$$\text{Productivity} = \frac{\text{Number of Secretaries x Average Typing Speed}}{\text{Number of Scientists}}$$

One interesting feature of this equation is that when the number of scientists is zero, productivity becomes infinite. This is probably true but it has never been tested empirically. People have discussed the question of whether a monkey hitting keys at random would ever compose a shakesperian sonnet. However, this is setting an overly difficult task for the average monkey. If we had asked that our animal type a scientific law, I am sure that he could manage it without too much trouble. For example, if a monkey were to punch

four keys at random over a long period, he would certainly hit upon $E = (MC)^2$ in less time than it took Einstein to reach it. Actually the odds of our monkey hitting it on the first try are only 42^4. If a monkey can do this, we should certainly expect more from our stenos, some of whom can type seventy words a minute. Think of the scientific laws that could be produced if four secretaries typed random letters at top speed. Some readers may feel that the current year's scientific output bears some resemblance to this, but I believe that a formal experiment would still be worthwhile. There is even a more economical alternative to having the monkeys type single letters, and this is to construct a machine that prints different combinations of scientific terms. The only work involved here is for someone to compile a list of the most frequently used terms which would be fed into the machine which would arrange and rearrange them in various combinations.

A research department without a secretary is indeed a sad spectacle. The scientists sit glassy-eyed in their offices, their dictaphones leering at them sardonically. Down the hall they hear the typewriters in engineering and personnel clicking away merrily. In their hearts they fear that others are writing more papers than they, that they are falling behind in the frantic race to amass a bibliography. Correspondence, both professional and personal, falls behind schedule. No one can find needed letters or data. A few persevering souls who learned to type as undergraduates are pecking out letters while their colleagues look on contemptuously and enviously.

Until that awful day when his own secretary leaves, a scientist never realizes how difficult it is to find a good secretary. The world is full of girls who can't spell and can't read their own shorthand. He has only to get one of these petite high school graduates to feel that the educational system of the country has failed miserably. Fortunately these girls work only until they get married and seem to know nothing about the intricacies of birth control. Thus a scientist will suffer for only a comparatively short period, at which time he must again approach the personnel manager for another girl. The inexperienced scientist imagines that the best secretary is young, attractive, and eager. A hardened veteran of a decade of research will always pick the fortyish housewife with married children. It is not only the scientist's wife who brings about this choice.

The experienced laboratory director always follows this rule--never let a highly trained stenographer do anything that can be done by a lesser trained scientist. The secretary's time is all important, and if she becomes burdened with adding columns of figures or tabulating data, then her typing and shorthand are bound to suffer. Competent scientists are a dime a dozen, good secretaries are hard to come by.

It is important to keep office distractions at a minimum. The best office for getting work done is one that is located at the end of a long basement corridor, and the worst place for an office is on the main route to the coffee shop. The best steno is one who doesn't have any friends in the building and isn't interested in local gossip. If the job doesn't require dictation, a deaf mute would be the ideal secretary; but if dictation is needed, then a mute secretary would probably suffice. Even more than other secretaries and errand boys, the greatest waste of the secretary's time occurs during the visits of the typewriter repairman. It is simply amazing how often typewriter

repairmen visit office buildings. In fact, it is very important to have two typewriters in every office so that if one breaks down, the secretary can switch to the other.

The greatest single source of turnover among secretaries is not, as some people think, marriage, but rather it is pregnancy. There is no necessary reason why a secretary must leave a job because she marries, especially if her employer exercises his good judgement in selecting a mate for her. Obviously, a geologist who will work in Kenya or an acrobat looking for a husband-and-wife team should be discouraged. However, there is nothing wrong with a secretary marrying a young man who (a) has taken vows of celibacy, or (b) does not want to raise a family. The wise employer should find it expedient to offer a type of baby bonus to all newly married stenos, but in this case it would be a bonus for not having children. This might be called a Barren Bonus. Even if all these measures fail, and of course, here we would include courses in birth control in the curriculum of secretarial training schools, the situation is still not hopeless. A secretary can be profitably employed until the ninth month of pregnancy. Even beyond that point she can still be provided with a portable typewriter for work at home and in hospital. Since it may be necessary to provide baby sitters for a steno after her child's birth, it would probably be most efficient to employ scientists' wives in this capacity. This will not only guarantee a steady source of baby sitters but also reduce the number of distractions in the scientist's home life, assuming that the baby sitting is done in the secretary's home.

In any event, one good secretary is worth two good scientists and the productive lab is the one with secretaries purring contentedly at their typewriters. Flowers on her desk, air conditioning, and electric typewriter--no expense should be spared to keep a good secretary happy. An electric typewriter will not only give her status in the eyes of her colleagues but also permit her to type faster and neater. Air conditioning, so long as steps are taken to see that other people do not visit the office frequently, will keep trips to the coffee lounge to a minimum. All in all, the wise laboratory director follows the principle, "productive research from productive secretaries."

TITLE: A Preliminary Study of Pseudo-conditioning under Conditions of Massed Stimulation in the Planarian*

AUTHOR: Paul Cornwell
Planaria Research Group
University of Michigan

In a recent study of classical conditioning with massed trials in the planarian it was pointed out that it is difficult to eliminate the possibility that Ss are being sensitized rather than classically conditioned (1). The present study was done in an effort to determine if planaria can be sensitized or pseudo-conditioned under conditions of massed stimulation.

SUBJECTS

Twelve planaria (D. tigrina) from the University of Michigan Tigrina Stock B were used. All of these Ss were the "descendents" of a single large tigrina. About 2-3 months before the start of the present experiment a single large tigrina was cut into 5-6 sections. As these sections regenerated they were cut again, so that within about 10 weeks there were 70-80 Ss in this genetically homogeneous colony.

APPARATUS

The apparatus used in the present experiment has been completely described elsewhere (2,4).

PROCEDURE

Two groups consisting of 6 Ss each were studied. Group SL received 150 shock trials which were followed by 150 trials of light. Group LS received 150 light trials which were followed by 150 shock trials. The duration of each light trial was 3 sec.; the duration of each shock trial was 1 sec. The inter-trial interval (ITI) varied from 10 sec. to 30 sec. The mean ITIs are given in Table I. Both the light and the shock trials were presented in blocks of 50 trials. A 5 min. interval separated each block. The light trials and the shock trials were separated by a period of from ten to sixty minutes.

Table I
Mean ITIs (seconds)

Grouped Trials

Group	N	Trials 1-125	Trials 151-275
SL	6	27.6	25.8
LS	6	21.0	27.8

*This research was supported by a grant from the National Institute of Mental Health.

The first 125 of both the light and the shock trials were run with 6 Ss in the trough at a time. During these trials no attempt was made to record responses. During the final 25 trials of each block (the test trials), Ss were studied individually. During the shock test trials (shock trials 126-150) all responses occurring during the 2 sec. immediately preceding the shock were recorded. During the light test trials (light trials 126-150) all responses occurring during the first 2 sec. of light were recorded. The mean ITIs for the test trials are given in Table II. When Ss were not being tested they were placed in individual aquaria and kept in a dark room.

Table II
Mean ITIs (seconds)

Individual Trials

Group	N	Trials 126-150	Trials 276-300	Conditioning Trials
SL	6	51	73	113
LS	6	49	75	114

Following the 300 shock and light presentations each S was given 25 classical conditioning trials. During these conditioning trials Ss were studied individually. Each conditioning trial consisted of a light-shock pairing. The duration of the light (CS) was 3 sec.; that of the shock (UCS) was 1 sec. The UCS overlapped the CS during the final 1 sec. All responses occurring in the initial 2 sec. of light were recorded. Half of the 6 Ss in both groups LS and SL were randomly assigned to each of the two delay conditions. Ss in one of the delay conditions received the 25 conditioning trials 1 to 4 hours after completion of the initial training. In the other delay condition the 25 conditioning trials were administered 19 to 25 hours after completion of the initial training.

If an animal remained motionless in the trough for more than 30 sec. it was prompted to move by water squirted from a pipette. The mean numbers of trials squirted per 25 test trials are given in Table III.

Table III
Mean Number of Trials that the Animals Were Squirted per 25 Test Trials

Group	Trials 126-150	Trials 276-300	Conditioning Trials
SL	1.67	4.0	6.84
LS	1.83	6.16	9.16

In brief summary, Ss in group LS received 150 light presentations, then 150 shock presentations, and, following a delay interval, 25 conditioning trials. The procedure was the same for Ss in group SL except that the order of the shock and light presentations was reversed.

RESULTS

The results of the shock test trials (shock trials 126-150) are summarized in Table IV. The difference in random response rate between group SL and group LS is not significant at the .05 level, using a two tailed t-test.

Table IV
Responses in 2 Sec. Preceding Shock on
Trials 126-150 (Shock Test Trials)

Shock Trials Preceding Light Trials		150 Light Trials Preceding Shock Trials	
Animal	Responses	Animal	Responses
SL-1	1	LS-11	1
SL-2	4	LS-12	2
SL-3	2	LS-13	1
SL-4	2	LS-14	1
SL-5	1	LS-15	0
SL-6	1	LS-16	9
*Mean	1.8	*Mean	2.3

*The means of these two groups are not significantly different.

With group SL it was noted that on the first 25-40 trials to the light alone there were many Ss contracting shortly after the onset of the light. Since 6 Ss were studied at a time during light trials 1-125 these contractions were not recorded. During light trials 126-150 there were fewer contractions observed. With group LS very few responses were observed during the first 125 trials of the light presentation period. A two tailed t-test showed no significant difference in response rate between group SL and group LS during the light test trials (light trials 126-150). The results of the light test trials are summarized in Table V.

Table V
Responses to Initial 2 Sec. of Light on
Trials 126-150 (Light Test Trials)

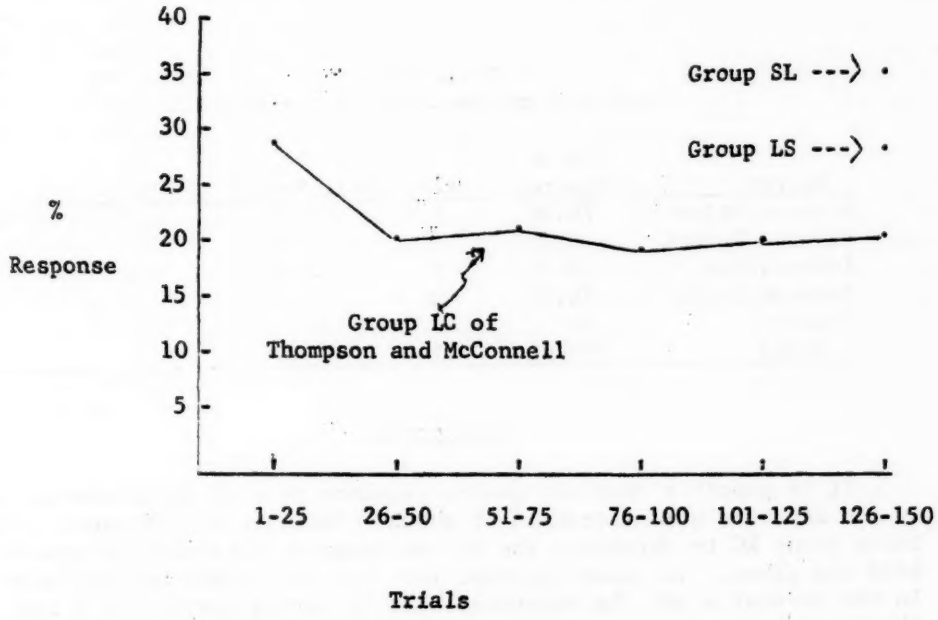
Light Trials Preceding Shock Trials		150 Shock Trials Preceding Light Trials	
Animal	Responses	Animal	Responses
LS-1	10	SL-11	5
LS-2	4	SL-12	12
LS-3	6	SL-13	12
LS-4	6	SL-14	7
LS-5	9	SL-15	13
LS-6	7	SL-16	6
*Mean	7	*Mean	9.25

*The means of these two groups are not significantly different.

It should be noted that during the light presentation period group LS received treatment very similar to group LC of Thompson and McConnell (4). These authors studied only one S at a time for the entire 150 trials, while in the present study group LS received the first 125 trials with 6 Ss in the experimental trough. For the light test trials group LS received the same treatment as group LC of the Thompson and McConnell study. A t-test showed no significant difference in response rate between group LS and group LC of Thompson and McConnell between trials 126 and 150.

Group SL is also similar to Thompson and McConnell's group LC, except that group SL received 150 shock trials before beginning the light trials. If shock does not sensitize Ss to light, then it might be expected that the response rate of group SL during the light trials would not be significantly higher than the response rate of the Thompson and McConnell's group LC. A two tailed t-test of the difference in response rate during light trials 126-150 between our group SL and Thompson and McConnell's group LC was significant at the .01 level. Figure I presents the response curve of group LC and the response rates for groups LS and SL during the final 25 light trials.

Figure I
Response Rates to Light Alone



The responses observed during the trials to light alone were typically rather slow turning movements. The responses observed during the 25 conditioning trials were typically strong longitudinal contractions of the entire body. The onset of the responses during the conditioning trials was usually at least a half second after the onset of the light. Table VI gives the individual results of the conditioning trials. A cursory examination of the data shows the Ss in group LS more responsive than those in group SL; the Ss

with the long delay between initial trials and conditioning trials were more responsive than the Ss with the short delay between initial trials and conditioning trials. A 2x2 analysis of variance was run on this data. The results are shown in Table VII.

Table VI
Number of CRs in 25 Conditioning Trials

Condition SL 150 Shocks--Then 150 Lights			Condition LS 150 Lights--Then 150 Shocks		
Animal	Delay 1-4 hr.	19-25 hr.	Animal	Delay 1-4 hr.	19-25 hr.
SL-11		14	LS-11		19
SL-12		20	LS-12		24
SL-13		14	LS-13		22
SL-14	14		LS-14	25	
SL-15	16		LS-15	17	
SL-16	16		LS-16	16	
Mean	15.3	16	Mean	19.3	21.7
S.D.	1	2.8	S.D.	4.1	2.2

Table VII
Results of an Analysis of Variance

Source	Sum of Squares	df	Mean Square	F	p
Between Orders	70.08	1	70.08	6.37	$.05>p>.01$
Between Delays	6.75	1	6.75	--	
Interaction	2.09	1	2.09	--	
Between Cells	78.92	3			
Within	88	8	11		
Total	166.92	11			

DISCUSSION

It is possible that the random response rate of the planarian is increased by the repeated administration of shock. Thompson and McConnell (4) employed their group RC to determine the random response rate when no stimulation of any kind was given. The mean response rate for this group was slightly under 15%. In the present study the responses made by our Ss during the 2 sec. preceding shock on the shock test trials might be considered a measure of the random response rate. The mean number of responses by groups SL and LS during the shock test trials were 7.2% and 9.2% respectively. These results, when compared with those of group RC of Thompson and McConnell, would seem to indicate that neither many exposures to light and then many exposures to shock nor exposures to shock alone cause an increase in the random response rate.

Results of the Thompson and McConnell study indicated that the light sensitivity of the planarian does not increase with repeated exposure to light.

In the present study, when responses to the initial 2 sec. of light were recorded, the response rate of group LS did not differ significantly from that of Thompson and McConnell's light control group. This could be considered a replication of the Thompson and McConnell light control results. It would also seem to indicate that having more than one animal in the trough at a time during light stimulation does not alter the response rate to light which would be expected if Ss were studied individually.

The results of the conditioning test trials parallel those of a previous study employing massed trials in the conditioning of planaria (1). The response rates tend to be higher if conditioning test trials are administered one day after the conditioning (or stimulation) session than if they are administered one to four hours following such a session. In the present study, however, the difference in response rates between the two delay conditions was not significant.

Pseudo-conditioning is defined by Hilgard and Marquis as "the strengthening of a response to a previously neutral stimulus through the repeated elicitation of the response by another stimulus, without paired presentation of the two stimuli" (2, p. 348). Thompson and McConnell apparently ruled out the possibility of planaria being pseudo-conditioned to light by being exposed to repeated shocks. Their group SC "was given a total of 150 shock trials, each lasting for 1 sec. After every five shock trials, 1 test trial was given which consisted of the presentation of the light alone for 3 sec." (5, p. 66). The results showed no apparent increase in response to the light after repeated shocks. An attempt to replicate the Thompson and McConnell SC group is currently being undertaken. To date, with an N of 6 animals, the results are in complete agreement with those of Thompson and McConnell.

Some of the results of the present study indicate that it is possible for planaria to be pseudo-conditioned to respond to light after repeated shocks, although not under exactly the same conditions as obtained in the Thompson and McConnell study. In the present study shock trials and light trials were administered in separate blocks, while in the Thompson and McConnell study light test trials were interspersed with the shock trials.

The following is a simple experimental design for a pseudo-conditioning experiment:

Group	Pseudo-conditioning trials (UCS alone)	Test trials (CS alone)
Experimental	Yes	Yes
Control	No	Yes

Pseudo-conditioning is said to have taken place if the response rate on the test trials is higher for the experimental group than for the controls. In the present experiment Ss in group SL received 150 shock trials prior to the initial light trials. Ss in group LS received no shock prior to the initial light trials. Although the responses were not recorded on the initial 125 light trials (6 Ss were in the trough at once) it appeared that animals in

group SL gave many responses to the light while those in group LS gave few responses to the light. This difference in responsiveness to light could be considered an indication that pseudo-conditioning was taking place in Ss in group SL during the exposure to shock.

On light trials 126-150 Ss were studied individually and responses to the light were recorded. On these light test trials worms in group SL had a higher response rate than those in group LS. This difference, however, did not reach the 5% level of significance. The failure to obtain a significant difference might have been due to the placement of the test trials at the end of the light presentation series. It is possible that a pseudo-conditioned response (PCR), present in Ss in group SL at the start of the light trials, had been partially extinguished by the 125 light presentations.

Following the 300 presentations of unpaired stimuli all Ss were given 25 classical conditioning test trials. Group LS had a higher response rate than group SL during the conditioning test trials. The difference between these groups might be explained by again considering the possible extinction of the PCR. If pseudo-conditioning extinguishes, then it might be expected that 150 light trials following the 150 shock trials would partially extinguish the PCR. Ss in group SL received this treatment prior to the conditioning test trials. Ss in group LS received no light trials between the shock trials and the conditioning trials, and consequently would not be expected to have had the PCR extinguished prior to the conditioning test trials. Thus, on the conditioning test trials group LS would be expected to have a higher response rate than group SL.

In the preceding discussion a heavy explanatory burden has been placed on the concept of extinction of a pseudo-conditioned response. Although such a concept sounds plausible, there is little empirical justification for its use. Nothing is known about the extinction of a PCR in the planarian, and not much more is known about the phenomenon in any other species (even in the white rat;). It would seem that research in this area would be in order.

As far as demonstrating that pseudo-conditioning can be produced in the planarian, the results of the present experiment are equivocal. In order to demonstrate clearly that repeated shocks can increase the responsiveness of the planarian to light, a group should be given 150 shock trials and then 25 test trials to light alone. The response rate of such a group during the light test trials should be compared with the response rate of a control group during 25 light test trials. Prior to the light test trials the control group would be allowed to glide about in the trough for the same length of time as it takes to administer the 150 shock trials to the experimental group. The control group, however, would receive no shock.

SUMMARY

Twelve planaria were given 150 shock trials and 150 light trials. Half of the Ss received the shock trials first, half received the light trials first. Following the 300 stimulus presentations, 25 classical conditioning trials (with light as CS and shock as UCS) were given to each S. The conditioning trials were given after two different delay conditions. Ss were studied individually only during the final 25 trials to light, the final 25 trials to shock, and the 25 conditioning trials.

It was found that Ss given light and then shock gave more CRs during the conditioning trials than Ss given shock first and light second. These results, along with other observations, have led to the tentative conclusion that repeated shocks can lead to pseudo-conditioning under conditions of massed stimulation. The design of an experiment to test this tentative conclusion was presented.

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TITLE: Planaria Regeneration as Affected by Temperature and Chemicals

AUTHOR: John Godwin
Thornton Township High School
Harvey, Illinois

1. Objective of project:

The objective of the project was to find out how certain chemicals and temperatures would affect the regeneration and reproduction of planaria. While I was studying invertebrates, I became interested in planaria and began thinking of doing a project with them. A statement in Collier's Encyclopedia gave me the idea of using chemicals in my experiments: "It also has been demonstrated that regenerative growth can be inhibited by the application of chemicals." The temperature experiment came into my project at the suggestion of my teacher.

2. Procedure:

The specimens were purchased from the General Biological Supply House three weeks before the experiment was begun. The planaria were kept in aerated water. Every five days they were fed beef liver, and the regeneration and number of specimens of each group were recorded. After feeding, the water was changed to remove the organic matter, and the correct amount of the particular chemical was again added if needed.

I used four chemicals in my experiments: colchicine emulsion, gibberellic acid, terramycin, and thyroxine. The two temperatures used were 44° F. and 70° F.

The chemical experiment using colchicine emulsion was begun first. Three cut planaria were put in water containing colchicine and three in water which did not contain it. Three cut planaria were also placed in water containing certain amounts of the other chemicals. Three were also placed in plain water to make a controlled experiment.

Amounts of Chemicals:

Colchicine Emulsion
1 part (.4%) solution was used per 1250 parts of water.

Gibberellic Acid
1 part of (15 milligrams per 28 cubic centimeters) solution was used per 250 parts of water.

Terramycin
1 part of (125 milligrams per 30 cubic centimeters) solution was used per 250 parts of water.

Thyroxine
1 part of (½ gram per 15 cubic centimeters) solution was used per 250 parts of water.

Three cut planaria were placed in each of two pans--one was kept at 44° F. while the other was kept at 70° F. The temperatures were constant.

In all but the temperature experiment the groups regenerated in about a week. Thus my findings are mainly based on the number of specimens in each group (increased by reproduction, especially asexual). A chart showing the number of planaria in each group every five days follows:

Days:	1	5	10	15	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90	95	100	105
44°	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
70°	9	9	11	14	14	15	17	18	20	23	27	29	32	34	41	47	52	55	57	60	69	80
Colchicine	9	9	9	9	10	10	12	12	12	12	12	12	13	15	15	17	19	20	20	20	21	21
Control	9	9	11	11	11	12	15	17	18	21	26	27	27	29	30	36	45	48	50	54	59	62
Control	12	12	15	15	17	18	20	23	25	26	26	28	33	35	35	36	38	40				
Terramycin	12	12	14	14	15	18	20	23	25	29	30	31	33	38	42	48	57	60				
Gibberellic Acid	12	12	13	15	16	17	18	19	21	22	22	23	25	27	30	30	30	32				
Thyroxine	12	12	15	18	18	19	19	21	24	27	31	34	36	39	43	45	49	52				

2. Results of the experiment:

The temperature experiment showed the most definite results of any of the experiments. At the end of a 105 day period the group at 70° numbered 80. The group at 44° numbered 9. The cooler group had only regenerated.

The colchicine emulsion experiment also had good results. At about the middle of the 105 day period a few of the specimens in colchicine were deformed--none in the group in plain water. Also the rate of reproduction began to slow down in the pan containing the chemical. By the end of the period all but one or two of the planaria were deformed. The number of specimens was also smaller: colchicine emulsion, 21, and the control, 62. Most of the specimens were long and thin--and thus were not perfectly coordinated. Two of the planaria specimens actually had a hole behind their pharynx--only one strand of each side held the two parts together.

The results of the experiments using terramycin, gibberellic acid, and thyroxine are rather indefinite. Each of the groups (including the group to which no chemical was added--"control") regenerated at the same rate. However, the reproduction of the planaria seemed to show some differences: control, 40; terramycin, 60; gibberellic acid, 32; thyroxine, 52. The specimens of the group to which terramycin and thyroxine were applied seemed to be larger and healthier on the whole than the control. The gibberellic acid group was just the opposite.

The greater number of specimens in the terramycin solution may have been brought about because the terramycin helped the animals to fight off microorganisms. This, of course, would make them healthier, allowing them to grow and reproduce. Of course, I cannot prove this--it is only a logical guess.

The results of the gibberellic acid experiment simply show what the chemical does to the planaria. At the present time the planaria look healthy, and on the whole they are average size. Their reproduction seems to be the only thing inhibited.

In explaining why the thyroxine group has a greater number of specimens than the control (52 to 40), we must look at the human child. If a child's thyroid gland is underproductive, cretinism results--this includes physical and mental retarding. Why couldn't growth be stimulated if there was an increased amount of thyroxine present in an animal where growth and regeneration are supreme?

4. Conclusions:

Concerning the temperature experiment I would say that the increased regeneration and reproduction of the warmer group (70°) took place because warmer temperatures speed up the metabolic rate.

Because of the length and shape and the reduced number of specimens in colchicine emulsion, I would conclude that this chemical inhibits the asexual (and sexual?) reproduction of planaria. The two specimens which have a hole behind their pharynx support this theory. This is the area where planaria constrict during asexual reproduction. It is the beginning of the so-called "sub individual". The chemical prevents the division of the animal.

In the other experiments I do not think that definite conclusions can be made. Terramycin and thyroxine may aid the health and growth of planaria, but more experiments must be made. Gibberellic acid may inhibit planaria, but here more experiments must also be done.

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World Book Encyclopedia

TITLE: Terns*

AUTHOR: F. E. Warburton
6171 Sherbrooke Street West
Apartment #16
Montreal 28, P.Q., Canada

Terns are a kind of bird with webbed feet. They are therefore water-fowl, like ducks and penguins, and so may be found in the same books as albatrosses and eagles but probably not in the same books as wrens and wrobins. This is because water-bird-watchers are so snobbish about dicky-birds.

Some terns are Arctic, some are Roseate, some are Black, and some are Caspian. In the tropics, some are Noddy, but nice. A few terns are Royal, but the majority are Common and proud of it--so proud of it that, in places, U-terns are prohibited.

Terns won't eat anything but fish, so it is no use putting out bits of suet and coconut for them in the winter: all you will get is dicky-birds, and you will have to buy a new book. Anyway, terns don't stick around in the winter. Arctic terns for example spend the summer at the North Pole. When it begins to get cold, they fly south to spend the winter at the South Pole, where it is summer. Having spent the winter at the South Pole, they fly North to avoid the winter, arriving at the North Pole in the summer while it is winter where they wintered. This remarkable migration, which involves flying more than 11,000 miles annually, was discovered when an arctic tern banded one summer in New Jersey was found floating, belly up, in the Congo River some other summer.

Terns are found in pairs, if they are good, because one good tern deserves another. They build their nests on the ground and places like that, and lay from 0 to 7 eggs. The usual number is 0, but many succeed in laying about 2, or sometimes approximately 3. The eggs are about the same size, shape and colour as those of other birds. They get sat on for a while, and sooner or later some of them hatch. (This is but a brief summary of the combined findings of many ornithologists; space restrictions preclude a description of the life history of terns in all its fascinating detail.)

Baby terns just a few days old are the cutest, fluffiest little things. They will sit on your hand just as friendly as anything, going "chirp, chirp" and looking at you with their big bright eyes and vomiting half-digested dead fish all over your shirt.

Our knowledge of terns is growing every day, as more and more research on them is carried out under the auspices of organizations like the Defence

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*Mr. Warburton is also the author of that famous expose of scientific "one-up-man-ship", 'THE LAB COAT AS A STATUS SYMBOL', which appeared as an editorial in SCIENCE a few months back. Readers should note that Mr. Warburton's present monograph on Terns is something of a satire, meant to be funny, which we have lifted bodily from THE MALPIGHII (circulation about 35, I think).

Research Board and the National Cancer Society, but there is still much to be learned. We can be confident, however, that one by one the problems will be solved. Science will not rest while yet a single tern remains unstoned.

TITLE: Note Concerning the Basic Health Research Institute
509 North Santa Rita Avenue, Tucson, Arizona

AUTHOR: Beatrice Gelber, Ph.D., President
Basic Health Research Institute

Basic research has expanded so rapidly that institutions of higher learning have been swamped by the demands made on them, to such an extent that chairmen of departments, who, in the last analysis, made the decisions, have wearily had to avoid considering the curtailment of facilities allotted to their regular staff in order to accomodate even the worthiest outsiders. The few basic research institutes now in operation seem to offer a good way of alleviating the problem of finding suitable housing for worthy research activity for which financing is readily available. Furthermore, scientists should not wait for wealthy donors to take the initial steps but should themselves assume the responsibility for organizing their own facilities to suit their needs. This can be done if the scientists will undertake the responsibility for starting research institutes and laboratories themselves, inasmuch as universities are no longer able to cope with the demands made on them in what has traditionally been their purview, namely the sponsoring, financing and housing of pure science.

With this need in mind, a small group of basic scientists formed the Basic Health Research Institute, a not-for-profit entity chartered by the State of Illinois. The Institute recently transferred to Tucson, Arizona and started operations in a small, rented building. The ongoing and immediately projected programs are concerned with cellular processes, considered from various viewpoints such as growth, fertilization, behavior, physiology. Techniques will include, among others, microphotometry, radioautography, microchemistry, electron microscopy, tissue culture, behavioral manipulations of protozoa.

The Institute will concentrate on quality. Additions to the group of researchers will be made only after careful study of the qualifications of those concerned. All members will already either have their own research grants or will have operated on funds supplied by similar sources. Each will find his own financing, though the sponsorship of the Institute will be available when applications are to be filed with grantor agencies. Overhead from grants is enabling the Institute to supply necessary facilities and services until sufficiently well established to enlist the interest of private foundations in such things as supplying buildings, equipment, endowments, etc. Some explorations in this direction have already been received with encouragement. In other words, we hope to be supported by government grants but not to be completely dependent on them.

For further information, please write me at the above address.

TITLE: Project Burrow: II. Selection

AUTHOR: John H. Hollis
Yerkes Laboratories of Primate Biology
Orange Park, Florida

In a previous research note on meal worms (Tenebrio molitor, larvae) Hollis (1960) presented data on 'time-to-burrow', using three media (bran, rice, and flour) and three illumination sources (7 w. red, 25 w. white, and 300 w. white). It is the purpose of this note to provide additional data on media preference.

The procedure followed was similar to that of Beck (1958) in which feeding preferences of European corn borers were investigated. In Beck's experiment the larvae were released in the center of a dish containing two pairs of different diets. Each pair of identical diets was positioned so as to be diametrically opposed on the diagonal. Preference was measured by counting the number of larvae feeding on each diet 24 hours later.

In the present experiment five media were used, providing a total of ten pairs. The following media conditions were provided as stimuli: (a) bran, (b) rice, (c) flour, (d) sand, (e) empty container. The paired media were presented in counterbalanced order.

The apparatus consisted of a clear square pyrex glass dish 20 x 20 x 5 cm. with a glass cover. In each of the four corners a 7 x 7 cm. 'critical region' was marked off. A white bottle cap 3.3 cm. diameter and 1 cm. deep was placed in the center of each critical region. Each pair of caps was filled to the top (level) with the appropriate media. The entire set-up was covered with an opaque cardboard box, which evenly reduced the illumination of the dish.

The experimental procedure was as follows: (a) ten Ss at a time were removed from their culture media, (b) the experimental media containers (caps) were placed in the 'critical regions' of the thoroughly cleaned dish, (c) next, the Ss were placed in a group in the center of the dish, (d) the dish was covered with a glass pane, (e) the entire set-up was covered with a cardboard box, (f) the Ss were checked aperiodically, until 80 per cent had moved into the 'critical regions', (g) the number of Ss in the 'critical regions' A & B were recorded, (h) the Ss were run successively on each of the ten media pairs, (i) this entire procedure was replicated five times with groups of ten Ss (Total N-50), with the order of pair presentation counterbalanced over replications.

In Table I the results are presented for the ten possible stimulus pairings. A total of 50 Ss was run on each pairing. Table II presents for each replication total responses to each of the six possible response categories. The percentages for categories were obtained by dividing each sum by the grand total. The response categories may be divided into two groups, nutritive and nonnutritive stimuli. The nutritive group was composed of bran, flour and rice. The nonnutritive group included sand and the empty container. There were three nutritive-nutritive pairs and one nonnutritive-nonnutritive pair. The remaining six pairs were formed of nutritive-nonnutritive combinations.

To determine if the responses to paired stimuli differed from chance expectancy a Chi square analysis was used (Siegel, 1956). For one degree of freedom a Chi square value of 3.841 is required for significance at the .05 level of confidence.

Table I
Responses to Paired Stimuli*

Number	Material	Ss Responding	Total Ss Responding	Per Cent	X^2
1.	Bran	40		82	
	Sand	9	49	18	18.36
2.	Bran	25		52	
	Flour	23	48	48	.02
3.	Bran	30		64	
	Rice	17	47	36	3.06
4.	Bran	41		85	
	Empty	7	48	15	22.68
5.	Sand	15		31	
	Flour	34	49	69	6.62
6.	Sand	18		41	
	Rice	26	44	59	1.12
7.	Sand	36		77	
	Empty	11	47	23	12.26
8.	Flour	32		68	
	Rice	15	47	32	5.44
9.	Flour	36		82	
	Empty	8	44	16	16.56
10.	Rice	35		70	
	Empty	15	50	30	7.22

* (a) Average length of meal worms = 2.82 cm.
(b) Average temperature at surface of dish 86.8°F.

The results indicate that three of the stimulus pairings did not exceed chance expectancy. These pairings included bran-flour, bran-rice, and sand-rice. Of the three pairs in which both members of the pair were nutritive media, only one, flour-rice, showed a reliable difference (X^2=5.44). Six pairings of the ten contained one nonnutritive member, and with the exception of sand-rice (X^2=1.12), all pairings differed significantly from chance. For the nonnutritive-nonnutritive pair, sand-empty, a X^2=12.26 was obtained.

A Friedman two-way analysis of variance by ranks was computed for the five materials and replications shown in Table II (Siegel, 1956). This resulted in a $X^2_r=36.72$, significant beyond the .01 level of confidence, indicating that the stimuli presented in this experiment were responded to differentially by mealworm larvae.

Table II
Replication Results

Replications	Bran	Flour	Rice	Sand	Empty	No Response	Total
1	27	22	22	13	10	6	100
2	25	27	20	18	7	3	100
3	26	20	21	17	8	8	100
4	30	28	21	9	5	7	100
5	28	28	9	21	11	3	100
Sum	136	125	93	78	41	27	500
Per Cent	27.2	25.0	18.6	15.6	8.2	5.4	100.0

These data suggest: (a) that the stimuli used were adequate, (b) that the paired nutritive media provided similar stimulation, i.e., Ss tended not to reliably select between pair members, (c) that overall the Ss responded differentially to the media. The failure of Ss to reliably select between members of paired nutritive stimuli appears to be a problem of generalization. In this case the results are most likely due to the similarity of the olfactory stimuli in question. It so happened that the nutritive stimuli selected for this experiment apparently did not lie sufficiently far enough apart on the olfactory sense continuum to provide different stimulus gradients. However, the worms do respond differentially to the stimuli question. From Table II it is clear that total responses tend to rank in an orderly fashion across rows (stimuli) and are relatively stable down columns (replications). Taking total responses as an indicator of selectivity, the worms prefer bran, flour, rice, sand, and empty container in respective order. In no case is there a reversal between ranking of sums for nutritive and nonutritive response categories.

It is somewhat difficult to compare these results directly with those of the preliminary report of Project "Burrow" (Hollis, 1960), since the former results are confounded with various types of illumination. However, in both cases bran ranks first. The results for the flour and rice unfortunately can not be so clearly stated and further experiments are necessary in order to compare the rate of burrowing with material preference.

The data tentatively suggest the following hypotheses: (a) that the nutritive and nonnutritive stimuli provide a stimulus gradient for the larvae, increasing in intensity toward the locus, (b) that the nutritive stimulus gradients are dominant over nonnutritive gradients, (c) that the stimuli of both classes provide the basis for a hierarchy of responses.

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An Added Note

In an article by Valentine (1930) forty-nine experiments are presented on the olfactory sense of the mealworm beetle. One set of experiments (nos. 36-41) concern reactions to food odor. Basically the food consisted of a broth made from raw bran flakes. His general conclusions were that the same sense organs were operative in the detection of the aqueous bran extract as were used for the perception of the female beetle. He placed the olfactory organs in the terminal four segments of the antennae. Dethier (1954) also suggests that the principle olfactory organs rest in the antennae of insects. There are, however, many sites of olfactory organs in insects (e.g. antennae, maxillary palps, tarsi) and it is Carthy's (1958) contention that the thresholds may vary according to site.

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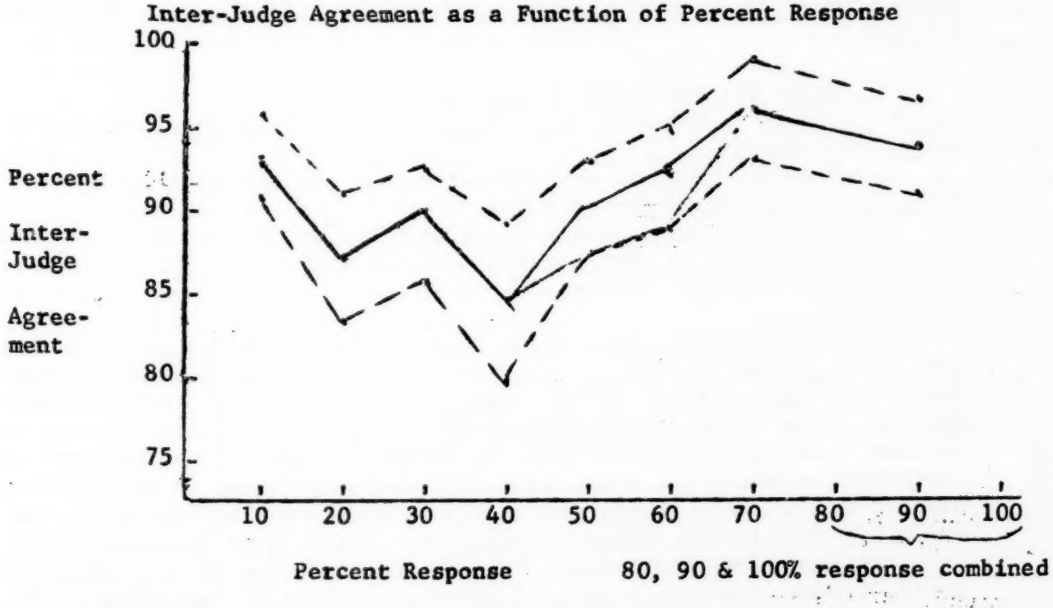
TITLE: Inter-judge Agreement in the Scoring of Conditioned Responses in the Planarian*

AUTHOR: Gretchen and Paul Cornwell
Planaria Research Group
University of Michigan

During a study of classical conditioning in planaria 680 trials were observed by two judges. One judge served as the experimenter, operating the controls for the onset of CS and UCS. The other judge watched the worm over the shoulder of the first judge. Both judges scored the responses made by S during the initial 2 sec. of the CS. (The CS was a decrease in illumination.) An attempt was made to conceal the recording of responses. At the end of each block of 25 trials the records were compared. The record of the judge who served as E was taken to be "correct" for purposes of computing the %-response of the S. For the next block of 25 trials the judges switched roles. Worms were used without the judges' knowledge of their conditioning history in an attempt to minimize experimenter bias.

The following graph presents the inter-judge agreement as a function of the %-response of the planarian. The dotted lines represent one standard error of the mean on either side of the %-agreement curve.



Discussion: It is encouraging to note that the %-agreement was consistently above 85%, and much of the time above 90%. For those determining conditioning on a trials-to-criterion basis it is especially encouraging that the inter-judge agreement is highest in the %-response region where Ss would be reaching criterion.

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TITLE: Towards a Theory of Two Kinds of Gene Change

AUTHOR: Thomas Morrill
1403 Stone Road
Tallahassee, Florida

Obviously a belief considered scientific is going to conform to the best evidence available. Scientists, while human and given to foibles in human relations, usually attempt to be ruthlessly honest with themselves and the evidence where their scientific theories are concerned. Faith and the authority of tradition should have no place in modern scientific theories. If this be true for theories in general, how much truer should it be for the most important scientific concepts, especially such a major phenomenon as organic evolution.

The foregoing is so obvious that almost no one would care to challenge it. But while faith and the authority of tradition should have no place in modern scientific theories, historians of science are quick to point out that in actuality custom and tradition can play an enormous role in keeping a scientific theory alive even when empirical data seem to challenge the theory. In this paper, I wish to point out a minor difficulty in the prevailing view of how genes change. I ask the reader merely to suspend judgment while we discuss a curious "bump" in current thought on the topic of evolution. The "bump" is as follows:

1. There are two types of gene change.
2. Type A is the only gene change recognized as important in standard evolutional theory.
3. However, standard evolutional theory has developed in certain respects quite apart from the observed data.
4. Meanwhile, gene change B, which has been discovered experimentally, seems in some respects more significant than type A, yet is not typically included in evolutionary theory.

As is well known, genes change (mutate) by encounter with electrons and other energy quanta existing in a "free" state outside normal organism function. The changes these energy quanta make in the genes are "accidental"--that is, there is no logical connection between the "quality" of the energy and the "quality" of the mutation it causes. Almost all scientists see this "mutation" as being the basic type of hereditary change in evolution. However, another type of gene change is equally substantiated by laboratory and other evidence; this is the change examined in detail in my previous DIGEST paper (1). Evidence for this second, "non-accidental" type of gene change is as sound, varied, and nearly as well known as is that of "mutation". The question now is not, "does this 'non-accidental' change occur," but rather, "what are its forms, what is its role?"

"Mutation" occurs when energy quanta disrupt the genes directly, thus causing a change in the organism's heredity. The second type of gene change (which, to distinguish it from "mutation," let us call "congenation") occurs as follows: Something in the organism's internal or external environment affects the organism; the organism responds in some fashion, physiologically, behaviorally, or otherwise; the response, or its after-effects, cause a change

of some sort in the genes, and thus a change in the organism's heredity. Evidence for "congenation" has been found by a variety of experimenters at various levels of the phylogenetic scale (2, 3).

Though a number of mechanisms have been proposed to account for the effects an organism's responses could have on its genes, the tendency remains in dealing with evolution to consider basic hereditary change only in terms of "mutation". Since the other type of change, congenation, does occur, the prevailing concept evidentally contains some unusual "immune" features not ordinarily present in the best of scientific concepts. Regarding evolutionary theory, we seem to think within a "circle of inhibition" which keeps us from seeing the other kind of gene change for what it really is. To explore this singular blindness so that the theory of evolution can be broadened to include congenation, we propose to follow in some detail the mutation concept's development from its beginning. We start with what seems to have been a major fallacy in Darwin's Origin of Species.

* * * * *

Darwin defined "indefinite variations" as being "the endless slight peculiarities which distinguish individuals of the same species, and which cannot be accounted for by inheritance from either parent or from some remote ancestor" (4). Thus was laid down the chief basis of the modern mutation theory. Occurring in individuals, not across groups, this change is fortuitous so far as the organism and particular features of the surroundings are concerned. It arises from the reproductive system's being "eminently susceptible . . . to very slight changes in the surrounding conditions" (4, p. 33). In Darwin's Animals and Plants Under Domestication, we find that "variability often depends . . . on the reproductive organs being injuriously affected by changed conditions . . ." (5). The Origin compares indefinite variability to the varying effects in different people of "the chill [in modern language, "the cold"] . . . causing coughs or colds, rheumatism, or inflammation of various organs" (4, p. 32). Under "changed conditions of life," we are told, "the reproductive system from being eminently sensitive . . . fails . . . to perform its proper function of producing offspring closely similar in all respects to the parent form" (4, p. 82). That system acts "irregularly," (4, p. 33) and "spontaneously" (4, pp. 250, 256, 270). External conditions are like a "spark" on "combustible matter" (4, p. 34). "Indefinite variability is a much more common result of changed conditions than definite variability . . ." (4, p. 32).

We began our history of the mutation concept with the Origin of Species, but we find the most explicit statement by going still farther back, to writings constituting the famous book's infancy. Darwin recalled in his Autobiography in June, 1842, "I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil in 135 pages; and this was enlarged during the summer of 1844 into one of 230 pages, which I had fairly copied out and still possess" (6). The 1844 Essay contains the following insight: "changes of the natural conditions of existence seem peculiarly to affect the condition of the reproductive system" (7). "Peculiarly" here can only mean that changed conditions affect the reproductive parts "characteristically" or "especially," other parts less so. At no point in the Origin of Species is the repeatedly-used adjective "changed" (conditions) clarified in a synonym. (Similarly Darwin never defined, except cyclically as "natural laws," the "natural" of

Natural Selection.) In the earlier Sketch we find: "accidents of air and water . . . bring rather suddenly organisms to new conditions . . ." (8). This confirms the Origin's opposition of "changed" with the "certain" conditions which cause definite variation: "changed" meant "accidental."

According to the founder of the modern view of evolution, the reproductive system was a hapless anomaly easily breached and disrupted by agents alien to normal function. The evolutionary "spark" was not struck in the organism, as such a dynamo of evolution; rather it arose in external-and-accidental environment, igniting "combustible matter." Its "proper function" being to copy the parent "form," heredity was a kind of self-running, though inefficient, factory outside the meshwork and living inter-play of organism and surroundings. To a lesser degree, Darwin did view heredity as change with organism response; such "definite," or as it is now called "Lamarckian," variation is entirely foreign to the modern mutation concept. As a matter of fact, Darwin profoundly misinterpreted the evidence.

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Indefinite variation is now known as effects of chromosome assortment, the shuffling of persisting hereditary differences in all plants and animals reproducing sexually. Union of one individual's sex cells containing one set of chromosomes with another's provides an immense number and variety of recombinations. Darwin erred in three dimensions: 1) chromosome shuffling does not involve in situ, fundamental changes, in genes. Rather than an isolated anomaly in individuals, it represents balanced transmission of racial traits in groups of organisms. 2) far from being caused by intrusive, external agents, chromosome assortment is geared to the developing organism milieu. It also involves complexly-integrated mechanisms in the cell nucleus. Accidents not only do not cause the phenomenon; it occurs in absence of, and in spite of very severe, traumas. The opposite of being "eminently susceptible," hereditary parts are least susceptible to changed conditions. And 3) the function of the reproductive system is not, as Darwin held, to copy the parent. That system produces differences, changing offspring from parents. "Reproductive" system is partially a misnomer; it is also an "individualizing" system.

Geniuses can be born into families of non-geniuses, from chromosome assortment; this has nothing to do with capricious change from accidental conditions, or with changes in genes. If intrusive agents do not cause better chromosome combinations, do they improve genes? We shall examine this problem in detail in light of later developments. Now we may ask whether Darwin "just happened" to overlook actual mutation in basing his theory on chromosome combination? On the contrary, Darwin knew about mutations and rejected them. Terming them "monstrosities", Darwin described mutations as "some considerable deviation of structure, generally injurious, or not useful to the species. . . . I have as yet failed to find, after diligent search, cases of monstrosities resembling normal structure. . . . their preservation would depend on unusually favorable circumstances" (4, pp. 58-9). That actual accidental gene change did not affect the subsequent growth of the mutation concept will be shown in our review of the later three stages: the Weismannian, the De Vriesian, and the Mullerian. First let us see further evidence that in the Darwinian beginning, the accidental-beneficial concept was itself no accident.

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It is generally held nowadays that very little was known about what Darwin called "endless slight peculiarities" before discovery of Mendel's laws in 1900; thus the error was not of traditional, or of willful, ignorance, but of simple ignorance. Sir Gavin de Beer in his foreward to the 1958 re-publication of Darwin's Sketch and Essay regards as total this lack of knowledge in the early 1840's: "First and foremost, no objective facts of any kind were known on the nature of heredity, and all that Darwin had to work on was the surmise of 'blended inheritance' according to which the characters of offspring were supposed to strike an average between those of their parents" (9). Yet in the 1860's, as de Beer tells us, "the solution of the problem of heredity and of the origin of variation was being provided by Gregor Mendel, and it was published in 1866;" this however was "unknown to Darwin and to everybody else for years . . ." (10). Looked at with a skeptical eye, there is something in this whole business which is not precisely straightforward.

If we accept that only an unknown, cloistered priest knew the "origin of variation" in Darwin's lifetime, we could perhaps go along with de Beer in overlooking his hero's lack of knowledge. But then what of Darwin's failure to suspend judgement? What of his presumption that biological incompetence underlay individual differences, an assumption which he offered the world in a scientific theory of evolution? We seem justified in inquiring, how obscure in Darwin's day was "the solution of the problem of heredity and of . . .variation"? Was there no observable evidence at all, except in Mendel's peas? What of the scientific literature--was Mendel really an isolated prodigy? And what of Darwin's own writings--did he recognize only blending, averaging inheritance?

Even in Darwin's time there were males and females; a man and a woman did not have "blended" children. This most impressive of hereditary variations is obviously not "irregular," "spontaneous," from accidental conditions. Most animal species, including man and Darwin's oft-cited pigeons, have 1-to-1 sex ratios. Identical twins should have been a clear indication of hereditary integrety. Supposed to have been so open and thorough in disposing of, as he called a chapter in the Origin, "the difficulties of the theory," Darwin never even mentioned the striking exception of one-egg twins to his key assumption, on which turns his whole theory, of "endless slight peculiarities."

As would be expected of the discoverer of a scientific solution to the greatest problem of science, Darwin was an extensive, meticulous experimenter; he was also a constant reader of biological journals. His books abound with references from the literature. Yet the predictableness of chromosome transmission was known in Darwin's lifetime; it was not only "in the air", it was "in print." The eminent German biologists Virchow and Haeckel both read Mendel's paper, and a copy of "Versuche ueber Pflanzenhybriden" was in the Royal Society of London library (11). In fact, two papers by Mendel on chromosome shuffling were published, the second in 1870, two years before the Origin's final, sixth edition. A perusal of Roberts' Plant Hybridization Before Mendel (1929) discloses that segregation of ancestral traits was known to at least a dozen other publishing experimenters before and during Darwin's life. As early as 1826 the Annales des Sciences Naturelles recorded these words by Sageret, a plant breeder whom Darwin quoted both in the Origin and the Animals and Plants: "Thus fusion of characters may take place in certain cases . . . [but] in general, the resemblance of the hybrid to its two ascendants consists not in an intimate fusion of diverse characters, . . . but rather to distribution, equal and unequal, of the same characters" (12). The matter was put even

more plainly when three years before publication of Mendel's now-famous paper Charles Naudin defined as follows his "law of disjunction": "All these facts are naturally explained by the disjunction of two specific essences, in the pollen and in the ovules of the hybrid. A hybrid plant is an individual in which are united two different essences . . . which mutually counter one another, and which are incessantly in a struggle to disengage themselves from one another" (13). The Animals and Plants Under Domestication refers to "Naudin's disjunction of characters" (14). In an exchange at the centenary of the Natural Selection theory, Professor Graham Cannon challenges as follows de Beer's whitewash of Darwin's blending:

> We hear such a lot about his experiments in his garden. He was always walking about tending his hybrids. But Darwin had before him results showing that the idea of blending inheritance was wrong. In 1799, that is, just ten years before Darwin was born, Andrew Knight showed that when tall and short peas are crossed, whichever way the cross is carried out the offspring are all tall. This he published. . . Darwin referred to these experiments in four separate places in his book on Animals and Plants Under Domestication, and yet did not see their significance. (15)

Evidentally the firmness and universalness of belief in blending arose after, and because of, Darwinism, and was not altogether its prior-de-terminer. This is the testimony of Darwin's own writings.

De Beer quotes the 1844 Essay's recognition of the fact, repeated in the Origin (4, p. 282) that "when two well-marked races are crossed, the offspring take more or less after either parent or are quite intermediate between them. . . In the second and several succeeding generations, the offspring are generally found to vary exceedingly . . . and may revert nearly to their ancestral forms" (16). (De Beer comments tenderly: "If only he had been in a position to realize it, here was the disproof of 'blending inheritance'.") Darwin's later publications leave no doubt that he pushed the concept of spontaneous variation in spite of his own accurate view. The Animals and Plants states: "the dissimilarity of brothers and sisters of the same family . . . may be in part accounted for by the unequal blending of the characters of the two parents, and by the more or less complete recovery through reversion of the ancestral characters on either side" (17). The Origin records that ancestral traits persist unseen until they appear "in one individual suddenly . . . after . . . some hundred generations" (4, p. 151). Perhaps the Origin's most effective argument against "special" or "fixed" creation was, 'domestic breeds evolve from wild species because they revert to them.' In the memorable case of pigeons, if "two or more distinct breeds [none having the dark wing bands of wild rock-pigeons] are crossed . . . the mongrel offspring are very apt suddenly to acquire these characters" (4, p. 44). But re-appearance of the dark bars involves disappearance of the breed marks! Darwin is saying the variations he has given as basic in evolution are "very apt" not to be basic. Blending would be equally fatal to the theory if it did occur; traits arising in individuals would be blended out of existence in repeated generations.

If Darwin knew blending was unequal, and long-term ancestral traits appeared in new combinations, how did he know there was the variation from external conditions? How did he distinguish the one from the other? Manifestly one could not separate differences from sudden reversion to unknown remote

ancestors, or from unpredictable, "unequal blending," and those arising "spontaneously" and "irregularly." Darwin had no way under the sun of knowing that any of his "endless slight peculiarities" were accidental and de novo. In fact none was; actual accidental changes were seen as abnormalities and rejected.

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It is usually assumed, and was assumed by August Weismann, that he discovered "the continuity of the germplasm." Certainly the German cytologist-geneticist made this important principle well known. However, "continuity" as he adduced it had the connotation, "isolation," and this principle was Darwin's. Weismann was Darwin's devoted follower; The Germ-Plasm (1893) was dedicated to the Englishman's memory. Weismann only made Darwin's severance of reproductive system from organism explicit and absolute; he made Darwin's "much more common" variation the only possible kind. Yet Weismann possessed the facts for understanding the derivative nature of these variations.

Evidence in cytology and genetics had accumulated rapidly in the 1870's and 1880's. Almost the entire microscopically-visible mechanism was known, including chromosomes and genes. Weismann had seen "chromosomes;" their name and picture are in The Germ-Plasm. His "ids," subsidiary condensations, linearly within chromosomes, we now call "genes." Ids contain "all the determining elements of the species, but in a manner peculiar to the individual" (18). Calling himself the first to describe "the reducing division of the germ cells," he wrote in The Germ-Plasm, "The germ-cell must in all cases by division get rid of half of its nuclear rods--that is to say of its germ-plasm,--in order to become capable of fertilization" (19). Individual differences arise from "the nature of the combination of the parental ids . . .", while identical twins come from the same egg (20). Toward the end of the book he summarized (my italics): "those specific variations which already exist in a species may be continually blended in a fresh manner, but it is incapable of giving rise to new variations, even though it often appears to do so" (21).

If every individual varies from assortment, how is one to know that any variation, in a given individual, is really a "deeper variation" produced by "direct effect of external influences"? (22) Only if it occurred in one of identical twins could the deeper change be distinguished, and Weismann made no attempt to do this. Like Darwin, he offered no criteria whatever for separating the two types; he pointed to no examples, gave no experimental evidence whatever. With a perfectly adequate explanation of his facts, he supported an additional, entirely different explanation of the same facts.

In establishing the germ-coma dichotomy for which he is famous, Weismann offered an experiment--that of cutting off tails of mice and observing no inherited effect--which, other than showing that mutilations are not inherited, has long been discredited. It tells nothing whatever about evolution. This experiment, in fact, had been discredited in advance by Darwin. According to the very "germ track" principle Weismann was claiming discovery of, mutilations did not figure under Darwin's Pangenesis because "gemmules formerly derived from the [missing] part are multiplied and transmitted [in the gonad] from generation to generation" (23). Growth of an organism from gemmules released from body cells depends "on their union with other nascent . . . units. They are believed to be capable of transmission in a dormant state, like seeds in the ground, to successive generations" (24).

Something other than "simple ignorance" underlay the Weismannian phase as well as the Darwinian origin. Darwin's and Weismann's approach was less one of ignorance than of ignoring. Their principles over-rode the preponderant evidence of the controlled nature of inheritance. Why did it take thirty-four years to discover Mendel's laws when Mendel had already discovered and published them? Apparently Darwin's revolutionary-though-scientifically-untenable principle had to be established by custom before the final truth of chromosome re-combination could be admitted. Darwinians clung to the belief that some assortment effects were accidental, therefore that accidental changes could be evolutionary, until Weismann proved that such changes were the only possible kind. Then to deny them would be to deny evolution.

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As early as 1889 a Dutch botanist, Hugo De Vries, discerned in the hypothesis of Pangenesis "the main principle . . . of unit characters" (25). ("Pangene" was later shortened to "gene.") "Specific characters of organisms," he wrote, "are composed of very distinct units" (26). Then in a 1900 paper, "Sur la loi de disjonction des hybrids," De Vries expounded what came to be known as Mendel's laws. Though he had by then read Mendel's 1866 paper, he learned of the Austrian monk's principles through his own study of hybrid plants. One of the first plants studied was a native of America, the evening primrose, Oenothera lamarckiana. In perhaps the best demonstration of the collective conscious in the history of science, two other men made the same discovery independently, announcing it, as well as their discoveries of Mendel's paper, in the same year. This not only confirms that the nature of assortment variation was ascertainable from study of organisms, it also shows that Mendel's paper was known, to be raised like a marionette on strings when the conscious stage was ready. Also we see collective manipulation of the ideas of Darwin. After general acceptance of "indefinite variation" as the only possible change, discovery of chromosome assortment came by way of Pangenesis, a theory Darwin intended would support the "definite variation." This discovery allowed the "indefinite variation" to become the more definite "mutation;" within a year, in 1901, De Vries published Die Mutationtheorie.

Rejecting chromosome shuffling as not basic, De Vries described its effect as "fluctuating" (often quite predictably), and "not new or original" (27). Re-combination effects became known as "Darwinian variations," their "non-heritability" as Professor Zirkle writes, being "in sharp contrast to [that of] De Vriesian mutations" (29). With the important step from chromosomes to genes Darwinism became neo-Darwinism.

De Vries's lamarckiana variations were unusual; this should have been a warning not to draw, as he did, his broad conclusions all from this single species. Particularly was this the case since that species was the very one which first led him to reject Darwin's variations. That "species," in fact, was itself a hybrid, and what De Vries observed was not mutation but an unusual case of the old Darwinian variation: "The mutants of Oenothera are therefore nothing more than symptoms of ... . peculiar hybridity and so as such of little significance in evolution" (Darlington and Mather, 1949, 29). Darwin's misjudgment not merely survived in Weismann and De Vries; it became chemical, in genes.

* * * * *

As early as 1913 the geneticist William Bateson questioned whether De Vries's "mutations" were not "an effect of the recombination of factors" (30). He remarked the hybrid origin of lamarckiana along with the "very curious" feature of the Oenotheras that they have "a considerable proportion of bad and shriveled pollen grains." By 1926 in papers published by Cleland (31, 32), evidence was complete that De Vriesian "mutation" was not mutation, that accidental hereditary change was back where Darwin left it, a monstrosity. As a scientific concept, Darwinism had been twice-shattered by the evidence of hybrid inheritance. As it was, Cleland did not mention his refutation of De Vries, and his papers caused little stir among biologists. Still something dramatic and radical was needed if neo-Darwinism were to retain its dynamism, resisting the blows of evidence. In 1929 something radically dramatic did happen; in that year, Professor H. J. Müller announced the first production of mutations in the laboratory.

Almost with its discovery, high energy radiation was known as a most pernicious destroyer of living organization. Only if an organism were cancerous could it be benefited by radiation, and even then the effect was destructive, of the cancer. When Müller exposed fruit flies to x-ray, no one, it would seem, would have thought of this in any sense as being more than the inaugurating of a new research tool, comparable on the gene level to extirpation of a body organ to learn of its function by its absence. So far as evolution was concerned, the experiment would seem to have been merely a replication in genes of Weismann's tail amputations; as taillessness was not inherited, so gene "amputations," while heritable in laboratory bottles, would not contribute to evolution in the wild. That x-ray is a typical mutagen is the sense of Professor Dobzhansky's (1941) remark: "The 'spontaneous' mutations, in so far as they are not produced by ionizing radiation present in nature, may be due to [radiation from] the thermal excitation of atoms which can occasionally exceed the energy of atomic cohesion" (33). Listing the common experimental mutagens, Conway Zirkle (1959) states: "All these mutagenic agents are destructive . . ."(34).

"Regarding the types of mutations produced," Müller reported in 1927 (35), "lethals . . . greatly outnumber the non-lethals producing visible morphological abnormality. There are some 'semi-lethals' also . . . ." Other types were: " 'invisible' mutations that caused an even lesser reduction of viability;" "dominant lethal genetic changes;" and finally, "mutations . . . which, when heterozygons, cause sterility . . . ." Discovery of mutation on a large scale was not the source of renewed and extended belief in evolutionary mutation because all the mutations Müller reported were deleterious! In 1931 Professor Sewall Wright declared: "actually observed gene mutations seem about as unfavorable as could be imagined for adaptive evolution" (26).

It will be recalled that Darwin compared indefinite variation to effects of a disease, "the chill;" it occurred when the reproductive system "fails . . . to perform its proper function," and is "injuriously" altered, when faced with changed conditions. The modern mutation concept fulfills this predilection of Darwin's. "Well over 99 per cent of mutations are harmful!--Müller (37). "The vast majority of known mutations are inadaptive" is the belief of Professor Simpson; "They are almost always disadvantageous . . ." (38). Huxley believes "1 in 100,000 is beneficial" (39). And even the rare mutation pointed to as beneficial is harmful.

Thus Negroes in the worst malarial districts of Africa are said to be better off because of abnormal red blood cells; the plasmodia are unable to live in these cells, the result of a deficient gene, fatal when homozygous. The condition is a disease, sickle cell anemia. Making the mutation advantageous is a harmful environmental agent, malaria. The writer worked one summer in a fruit cannery where the only truly adapted worker was one who stood ten hours a day in a box car loading empty cans on a conveyor belt; he was feeble minded. Where there is a failure in evolution, loss of additional capacities may permit short-term survival; species often become extinct by gradual attrition. But evolution does not seem to consist mainly of survival of less efficient organisms in poorer environments. Defining evolutionary mutations as those 'enabling organisms to survive in deleterious environments' only proves the harmfulness of mutations. A test mutations must pass in evolution is: does the mutation increase the gene's reactibility? On this point the evidence is overwhelmingly negative. Should a mutation pass this test, a second equally formidible test awaits it: If it resulted in more reactible genes, could accidental gene change lead to more reactible species?

As set forth by Darwin, Natural Selection worked directly on the variable, the individual organism. A "grain in the balance" decided which lived, which died. Mutations, however, are in single genes; and are not selectable over series of generations because they are swamped by chromosome assortment (40). Empirically there is this fact: Evolution has been most rapid under conditions least favorable to mutation-selection, that is, in species having the lowest "number of conceptions per number reproducing," as in the human line. Furthermore, unlike Darwinian variations which are random, mutations are "well over 99 per cent . . . harmful."

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It seems to the author that as far as evolutionary theory is concerned, the gene mutation is considerably over-valued. At the same time there needs to be a corresponding recognition of the directed gene change, which in this paper has been termed "congenation." Just what the relative importance of each change is overall, where in evolution each is more effective, will then be a major question for future scientists to decide.

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THE WORM'S-EYE POINT OF VIEW

(The title of this feature is taken from Ernie Pyle's HERE IS YOUR WAR: "I write from the worm's-eye point of view.")

FROM: William F. Carlock
Advanced Instrumentation
Research and Development Group
The Martin Company
Denver 1, Colorado

I have worked, on an amateur basis, with conditioning effects on second generation planaria off and on for a number of years. My experiments involved training planaria to make a left turn in a Y tube in response to increasing liver juice concentration in the left hand fork of the Y tube. When conditioned, they would be cut in two or three parts, allowed to grow and the conditioned response again tested. I found the training in evidence in the second generation, though I would not want to support my conclusions on a strictly scientific basis.

(It is letters such as this one, pointing out how much of our own work was anticipated by other scientists, that make us appreciate the theory of the Zeitgeist in science. We are pleased to learn of Mr. Carlock's early work and hope he will continue his experiments and keep us informed of his results.--Ed.)

FROM: Don Kirsch
9429 S. 83rd Ct.
Oak Lawn 4, Illinois

Thank you for sending me the August, 1960 WORM RUNNER's DIGEST.

If you recall, LeRoy Neitzel and I did a science project on "Conditioned Responses in Planaria". We were very happy to see one of our letters published in the DIGEST.

We had a bad turn of luck at the Illinois State Science Fair and only received a second award. It seemed as though one of our judges had some sort of grudge against planaria and suggested that we work on something else like amphibians. This was rather discouraging, after finding so many reasons for working with the worms. However, we had many other encouraging remarks on our project. We had several long talks with people who were very interested in our work. Among these was Max Braverman from the University of Illinois, whose letter you also published in the DIGEST.

We were also able to build up a conditioned response with the automatic switch controler after following your suggestions. However, we were only able to get about ten CR's out of twenty-five trials, which is not too good, but shows that there are possibilities for the controler.

LeRoy and I are also considering continuing our project for this year's Science Fair. In this way, we could further our research on the worms and add to our project.

FROM: Robert [illegible]les
Division of the Natural Sciences and Mathematics
Department of Psychology
Hollins College, Virginia

Somehow I got on the list for the WORM RUNNER'S DIGEST, which I don't mind at all. This is probably the best edited, at least, journal in the field. All sorts of esoteria there. I would like to acquire back issues if there are any available. I also have a quote, or at least as well as I can remember it. It's from Russell's New Hope in a Changing World, I think:

> Over the course of evolutionary time we can witness the worm becoming a philosopher. This is progress. Unfortunately, it is only the philosopher who tells us so.

(As we indicated at the beginning of this issue, back issues of the DIGEST "sold out" like flatworms at a science fair. We hope eventually to hit upon some means of making the first two issues available again, but at the moment cannot supply them even to good friends.--Ed.)

FROM: Ruth Ziegler
3107 Crockett Avenue
Roanoke, Virginia

Help! I have racked my brain, read books and papers, and lost sleep to the limit. Here's the question. How does one safely administer a drug to a planarian?

I am planning a study of the effects of a depressant, GABA, on conditioning. The big snag is how to get it into their digestive tract or nervous system safely and regulate the amount at the same time. Injections don't work. I just ran three planaria through.

I would deeply appreciate any answer to my frenzied cries as soon as possible.

(If we knew the answer to this problem, we'd all be in better shape than we are now. We too have "run planaria through" in our attempts to inject chemicals into them; nor have we had much better luck in attempting to immobilize the animals by placing various anesthetics in their water. Either the chemicals are so weak the worms still wiggle, or if strong enough to keep the worms quiescent, do so "permanently." But dead worms are of little use in testing "retention." Bill Corning has had good luck with chemicals--see his paper in the current issue--but to the best of our knowledge, no one has ever hit upon a satisfactory way of injecting chemicals or of precisely regulating the amount of chemical "taken up" by the worms from their environment. Any suggestions from our readers?--Ed.)

FROM: Robert Ross
Delta Phi House
Williams College
Williamstown, Massachusetts

[illegible]

I would like to commend you on your pioneer work in the cause of humor: THE WORM RUNNER'S DIGEST. I can see Mad Magazine re-examining its basic philosophies. I would like to commend you, furthermore, for the work you are doing with planaria, thus making a place in this world for our little-appreciated Turbellaria friends. My father thanks you, my mother thanks you, my sister thanks you. Or, in the deathless words of Virgil, "Quos Ego."

Please put me on the mailing list.